

Psychological Bulletin

HARRY HELLSON, Editor
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Psychological Bulletin

MOTIVATION AND THINKING:

A SYMPOSIUM

W. EDGAR VINACKE

University of Hawaii

The current period in psychology is marked by the vigor with which theory and investigation are coming to grips with dynamic aspects of behavior. To a very significant degree, conditions which have been visible mostly in the clinic or the case study are becoming conspicuous variables in experimental research.

Presently, too, there is a rapidly mounting renewal of interest in human thinking, mostly in the areas of problem solving and concept formation—but with indications that imaginative processes are also becoming respectable again in the laboratory (with an empirical orientation, that is, rather than from the practical standpoint of projective techniques). The great attention to creative thinking in recent years is another important indication of this trend.

It is often the case that streams of investigation well up alongside each other, with their separate preoccupations and their own technical elaborations, probably springing from different sources (those fascinating innovating springs in science). They may long continue to run happily in different channels with little direct interfusion. So, to a considerable extent, it appears to have been with those psychological specialties called "motivation" and "thinking." We can all cite well-known names in each, but without, as a rule, placing a person in both at once. Yet thought-

ful psychologists are fully aware of how artificial these distinctions really are.

I have come increasingly to realize that there has been progressing for some time a courtship between motivation and thinking. Although the suitors may be coy, at times, or even scornful of each other, nevertheless on the whole they have been approaching their wedding day with surprisingly clear intimations of harmony. I refer, here, to academic experimental psychology, not to those traditions (like psychoanalysis) where no division has ever really existed between dynamic and cognitive aspects of behavior.

This symposium brings together a diverse set of papers. The reasons for this diversity are easy to see. If you seek for rapprochement between the fields, known as motivation and thinking; you will soon discover that a wide variety of psychological work, theoretical as well as experimental, belongs together. The four papers, therefore, symbolize this mutuality of interest just as much as they represent widely different approaches. They reflect the sort of effort that must be made if a happy marriage between motivation and thinking is to be consummated.

We shall, therefore, let the papers speak for themselves.

(Received June 21, 1961)

THE COMPLEXITIES OF THINKING¹

W. EDGAR VINACKE

University of Hawaii

The general problem with which this symposium is concerned is certainly very basic in psychology—and it is one that I personally have found increasingly important as the years go past. Indeed, in my opinion it lies at the very heart of human behavior. This problem may be stated as the degree to which cognitive performance varies in content, quantity, and quality with variations in the motivational state of the individual. By “motivational” I mean “the . . . processes that instigate, regulate, and adjust behavior” (Vinacke, 1960b). One may prefer to conceive of such conditions as inferred intrinsic properties of the person—or he may cautiously speak only about the manipulations of the experimenter, such as his instructions or the kinds of pressures (often called “stress”) that he imposes upon his subjects; for our purposes, either language has to do with motivation.

Of course, psychologists by no means agree as yet, if they ever will, on answers to the difficult theoretical issues that arise when one invokes motivational determinants of behavior. These issues concern the role played by driving as compared to steering aspects of the motivation

system, the proper way to explain “primary” and “secondary” components of motivation, and so on. Recent books by Bindra (1959) and Brown (1961) have sought to clarify these issues, and I, too, have written about them (Vinacke, 1960a). As so well revealed by Hall and Lindzey (1957), personality theory has sought in the past and still continues to seek for a satisfactory account of motivation. Although it is extremely tempting to employ this forum as a means to present that definitive treatment of motivation for which you have all been waiting, I must disappoint you; since, at the moment, I wish to speak about thinking rather than motivation itself. Whatever brand of motivation theory you prefer, I shall insist that thinking is at all times “the utilization of past experience in response to motivational states” (Vinacke, 1960b).

In general, therefore, my thesis is that thinking is inseparable from motivation. We cannot possibly advance our understanding of thinking without continued and intensive study of the motivational conditions which change its course and determine its character. In expounding this theme, I shall first look briefly at its emergence in psychology, and then indicate how data bearing upon it are accumulating at a rapid rate with the result that quite exciting vistas open for the future.

HISTORICAL ANTECEDENTS

There is, as you all know, nothing terribly new in the statements I have just made. We have only to recall the classical discussion of

¹ This paper and the following three papers by Maltzman, Johnson, and Hilgard were presented in a symposium at the Western Psychological Association meetings in Seattle, Washington, June 15, 1961, with W. Edgar Vinacke acting as chairman.

I am much indebted to David Crowell, whose thoughtful advice facilitated the writing of this paper. Work carried out under a fellowship from the John Simon Guggenheim Memorial Foundation provided essential background.

attention—of which Wundt's doctrine of apperception is an especially striking example—to see that interest in dynamic aspects of thinking has had a history parallel to that of scientific psychology itself. Psychoanalysis from the very beginning has seen thinking as depending upon motivational states, stressing unconscious determinants, as well as ego regulating processes. In Lewin's work, the dynamic emphasis was always strongly apparent, represented by concepts of tension and its resolution, of valence and vector. The influence exerted by Murray's studies of personality depends directly upon his having closely tied together cognition and motivation. Perhaps no one has more clearly and forcefully recognized the interrelations of motivation and thinking than Gardner Murphy. Indeed, his formulation of thinking as a continuum between autistic and realistic poles can scarcely be improved upon: thinking is a function of the onward-course of need-conditions which, however, are responsive to the character of the environmental situation.

I could go on reminding you of dynamic approaches to thinking, but perhaps this is enough to make the point clear that the motivated character of thinking is far from a new idea in psychology. Nevertheless, whereas clinical psychology is built directly upon motivational concepts, experimental psychology has only recently come to depend much upon them. Let us glance at these developments.

EXPERIMENTAL APPROACHES

Perhaps it is not an exaggeration to say that the past decade has witnessed a revolution in the psychological laboratory. One might say that not long ago the objective of an experi-

ment was to expose a standardized person to a situation in which stimuli were allowed to vary under carefully controlled conditions. That is, the ideal was to rule out individual differences in order that the effect of the stimulus could be observed in a relatively simple fashion. Now, however, the opposite arrangement is coming to be the rule—we standardize the situation in order to see how variations in subjects are related to response in that situation. Perhaps eventually we shall arrive at the point where we know how to allow both person and situation to vary in relation to each other; indeed, this paper is concerned with this very matter.

The necessary steps are rapidly being taken. A search of the literature reveals that prior to the middle 1940s only a miniscule fraction of psychological publications dealt with motivational variables—and most of these were concerned with the recall of pleasant versus unpleasant material, in what were conceived to be tests of the Freudian principle of repression. Rapaport in 1942 presented a thorough and critical review of this work, concluding that "emotional" factors had consistently been shown to influence the course of thinking, varying with the personal relevance of the material, as well as the intensity and quality of the emotional factor itself.

If we add research on the level of aspiration and the resumption or recall of incompleting versus completing tasks, we come close to exhausting experimental studies of motivation—at least with human subjects.

What a difference can be seen today! It has become almost as difficult to keep up with research on motivational variables as with that on learning. Three major procedures

have been devised to manipulate motivation: namely, deprivation, the assessment by pretest of latent motivational properties, and the use of induction to influence the subject in the situation itself.

Deprivation signifies the prevention of some activity, which may be considered consummatory, for some specified period of time. Although this technique is widely employed with animal subjects, it is still in its infancy with human subjects. The pertinent research has so far chiefly been concerned with hunger and with the withholding of sensory or social stimulation. In general, as deprivation continues, imaginative activity relevant to the inferred need state increases (Sanford, 1936) at least up to an optimum point, after which there is a decline (Wispé, 1954). This probable curvilinear relation (the "optimum principle"), formulated in 1908 by Yerkes and Dodson, is potentially of vast importance. Unfortunately, most experiments have not been designed to reveal it, since it is usual to constitute groups chosen from the extremes of a continuum. With respect to problem solving, however, it appeared in an experiment by Birch (1945). Chimps produced better solutions to stick problems instrumental to food under conditions of moderate deprivation.

Studies with sensory and social deprivation are so far mainly in the exploratory stages, but it is quite clear that there are important effects upon performance (Bexton, Heron, & Scott, 1954; Gewirtz & Baer, 1958) and an interesting avenue opens out here for future research.

More extensively developed in research on human subjects is the administration of pretests, by means of which to identify persons who differ in some respect, such as "achievement" or "affiliation," or manifest or

test anxiety, or perhaps some attitudinal variable like the "cognitive styles" studied by George Klein and his associates. It is by now familiar to everyone that thematic apperception pictures yield scores that may fruitfully be regarded as reflective of significant motivational patterns, a notion, of course, basic to projective testing. Much of the work initiated by McClelland (McClelland, Atkinson, Clark, & Lowell, 1953) and pursued actively by Atkinson, French, and many others, has been methodological in character. But it has been established that imaginative response varies with latent dispositions, as well as with experimental inductions. Beyond this, differences in performance on a variety of tasks are associated with differences in motives, as inferred from pretests. For example, persons high in affiliation are more efficient in tasks that require sensitivity to social cues than are persons low in affiliation (Atkinson & Walker, 1956; French & Chadwick, 1956).

The situation is more complicated with respect to anxiety measures, chiefly because it is probable that the effects of anxiety depend upon a considerable variety of additional conditions, like the complexity and difficulty of the task, and the kind of inductions employed by the experimenter. Nevertheless, a wide variety of effects associated with anxiety has been reported (Sarason, 1960).

Some of the most promising research has been conducted by Klein (1954) with attitudes that he calls "cognitive style." This is defined as a regulative process that determines attention to the goal and how a person copes with the task situation. In one experiment, constricted and flexible subjects were compared under thirsty and sated conditions. In a judgment task, flexible subjects over-

estimated size, regardless of degree of thirst, whereas constricted subjects underestimated. On the other hand, in a tachistoscopic task, there were no differences when subjects were sated, but flexible subjects were more variable, and also more accurate, under the thirsty condition.

Still a third procedure has been widely employed. This technique which may be called "induction" consists in the introduction of special conditions into the situation itself. Thus, an experimenter attempts to influence the subject's relation to the goal by creating perceptions of success or failure. Or noxious stress may be imposed by the administration of electric shock. Or verbal instructions may be given to induce ego-threat. In all of these cases, it is regularly found that conditions intended to be unfavorable—i.e., threat, failure, and so on—are likely to interfere with performance, whereas favorable conditions, especially success, facilitate performance. Cognitive processes, perhaps at a rather superficial level, are thus readily affected by experimental conditions.

LEVELS OF THE MOTIVATION SYSTEM

Rather than present more examples of the effects of specific conditions, I should like to indicate several of the complexities that emerge in current research. The single most important point to be made, in fact, is that cognitive behavior is an outcome of the interplay of many forces. Since I suppose no one has ever really doubted that this is the case, perhaps I should say that we are learning how to incorporate these complexities into our experiments. This is one facet of the revolution I mentioned before. Instead of designing simple, single-variable experiments, the necessity arises to plan multivariable ones. It

is possible, of course, to look into the future to a time when we may be able to return to simpler experiments by way of indexes to patterns of variables.

This may be illustrated by reference to achievement. As McClelland et al. (1953) point out, a high degree of achievement imagery may be a function (a) of a high latent disposition, or (b) of strong achievement cues in a picture-stimulus, or (c) of special orienting instructions designed to induce achievement. That is, one may affect achievement scores in any of three different ways, so that potentially we have three different kinds of "high achievement subjects." But there may be systematic relations among them. For one thing, of course, there may be correlations that could be established, although according to a study by Marlowe (1959), such relations may be small. Instead, it might be profitable to conceptualize categories of individuals high on pretest, but low on response to special cues and experimental induction; another category might be high on pretest and high under induction, but low on special cues; and so on (see, for example, Martire, 1956). One might then find a single measure that would permit the direct identification of persons who fall into each category.

In the meantime, however, we must recognize that the motivation system is highly complex, and that several variables may simultaneously require measurement. I find it meaningful in this connection to speak about "levels of the motivation system." We may draw inferences about several aspects of motivation—a point recognized in most theories of personality. A convenient distinction is in terms of instigation or motives (the relatively enduring and general forms of energy-expendi-

ture—what Judson Brown calls “sources of drive”), regulation or attitudes (the mechanisms which determine the course of activity once a change in instigation occurs), and adjustment or sets (the specific process that determines the immediate response itself). Of course, such distinctions are bound to be artificial, and, in making them, I ignore many important and interesting issues dear to the heart of the motivation theorist. Nevertheless, to differentiate such levels casts a clarifying light upon the character of thinking. A few examples must suffice.

Inferences about instigation may be based either upon conditions of deprivation (saying, for example, that the individual gets hungrier) or upon scores made on a test presumed to reflect latent tendencies like the TAT (thus, for instance, saying that a person is high in latent achievement). Two sorts of inference may be drawn: on the one hand, that a given condition enables us to compare a higher with a lower state of instigation—for example, individuals low and high in latent achievement, or satiated and hungry; on the other hand, we may compare one kind of instigation (what may be called “predominance” of a motivational tendency) with another kind, for instance, achievement versus affiliation. Both sorts of inference prove useful in research. Thus, the hungry chimp displays better instrumental problem solving than the satiated one (Birch, 1945), and the hungry college student seems to be more preoccupied with thoughts of food than the one who has just eaten (Wispé, 1954). The individual high in achievement will apparently solve more arithmetic problems under conditions intended to induce achievement than the individual high in affiliation, who works

better when the incentive is to “please the experimenter” (Atkinson & Reitman, 1956).

Inferences about regulative mechanisms may also be drawn from tests, which, in this case, seek to assess how a person typically handles a given kind of situation. The cognitive styles described by Klein belong in this class, as also do ego-defense and ego-strength processes, as well as values, attitudes towards problems and problem solving, and so on. That the identification of regulative processes aids in understanding thinking has already been illustrated in the reference above to Klein’s research. Another good example is provided in experiments by Schroder and Hunt (1957) and Scott (1956), who measured failure-avoidance tendencies. Individuals marked by this characteristic tend in problem solving situations to set unduly high goals, employ few alternative solutions, and to act in an unrealistic fashion.

Finally, it pays, also, to take the adjustment level into account, since, for instance, the conditions manipulated by the experimenter in the situation itself may have important effects upon performance. Of course, the literature on set, which could be adduced as evidence, is enormous; in fact, the psychologist is especially skillful in influencing this level of the motivation system. The extensive research on the induction of success and failure provides the best example. Particularly pertinent is the experiment by Lantz (1945) with school boys. After they played a game in which they experienced either success or failure, she administered Stanford-Binet items to her subjects. Failure had an adverse effect upon reasoning tasks, whereas rote memory was not affected.

This necessarily abbreviated dis-

cussion serves to demonstrate that complexity in thinking is a function of several inferably different motivational levels. Response depends upon the organization of these processes into patterns.

INTERACTION

One feature of this organization that deserves special mention because of its methodological implications, is the interaction of one level with another. That is, the effect of a process at one level depends upon the characteristics of another level. For example, it has become apparent that it is not sufficient to specify the degree of manifest anxiety alone because persons "high" in this respect, respond differently under different sorts of experimental instructions—that is, the adjustment level determines response as well as whatever process we infer manifest anxiety to represent (see, for example, Sarason & Palola, 1960).

A good instance is provided by Miles (1958). In this experiment, subjects high and low in achievement imagery were also classified as either "analyzers" or "nonanalyzers" on the basis of their approach to the Wechsler Block Design test. On a pursuit-meter task, high achievement analyzers proved to be superior to low achievement analyzers during the early stages of original learning, after which the latter performed better. In relearning, high achievement non-analyzers showed the greatest loss, whereas low achievement analyzers showed the least.

RELEVANCE

But, perhaps, a more fruitful way to look at the complexities of motivation is in terms of the relevance of the situation to the inferred motives and attitudes of the subject. In this re-

spect, the character both of the induction and of the task assume a clear significance. Although it is usual for the investigator to build relevance into his experiment, the true importance of this factor only emerges sharply, as yet, in a few experiments. The most revealing of these was conducted by Elizabeth French (1958). She used groups of four subjects, each group composed of individuals high in either achievement or affiliation. In addition, she induced individual compared to group orientation and also had the groups work either under task feedback (emphasizing the work situation) or under feeling feedback (emphasizing the interaction situation). In a task requiring the reconstruction of stories, the groups high in achievement were more efficient with task feedback, regardless of orientation, whereas the groups high in affiliation were superior under group orientation with feeling feedback.

Other contributions to an understanding of relevance have been made by Vogel, Raymond, and Lazarus (1959), Schönbach (1959), and others. In these experiments, it is shown that performance is better when the task is relevant to the inferred motive than when it is irrelevant.

Unfortunately, time does not permit a more detailed analysis of the many interesting questions touched upon in this paper. However, it is quite apparent that there is a complex relation between the quantity and quality of thinking and the motivational state of the individual. Experiments are rapidly showing how these relationships may systematically be explored. I am sure that we are approaching a time when the dynamics of thinking will be better understood.

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MOTIVATION AND THE DIRECTION OF THINKING¹

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Motivation is a difficult and complex problem in its own right. In conjunction with thinking, a so-called complex process, the difficulties of analysis appear to be immeasurably compounded. This as well as other problems of thinking, however, may be made more amenable to experimental and theoretical analysis if we assume that thinking is a complex form of behavior involving changes within and between classes of responses occurring on the basis of principles derived from simpler experimental situations (Maltzman, 1955). Such a position, of course, does not preclude the development of new concepts and principles which may be peculiar to thinking, but only that such conceptual additions be introduced when already established concepts are clearly insufficient for the task of analysis and explanation.

Before proceeding with the consideration of motivation and thinking, it would be advantageous to clarify our usage of some relevant terms with a simple illustration. A common experimental task employed in the study of thinking is anagram solving. The subject is presented with a series of jumbled letters such as a-p-h-e-c. His task is to construct one or more words using the letters presented. Presumably he has never

encountered this anagram before. The letters evoke corresponding verbal responses on the part of the subject which when rearranged spell the words "cheap" and "peach." A sequence of letter responses initially evoked must occur in a new sequence for a successful solution of the problem. This formation of new or different response sequences is characteristic of thinking as distinguished from recall. Learning in the form of acquisition of response strength by the particular solution is prohibited by having only a single presentation of this letter sequence.

A number of different parameters may influence performance in such a situation. For example, problem solving difficulty may be manipulated by systematically varying the letter order of the anagram. Subjects in such experiments are usually college sophomores who have repeatedly encountered the words peach and cheap. These responses have been well learned, are familiar, although not to the anagram. A measure of availability of the solutions may be obtained from the Thorndike-Lorge word count. Relevant learning of the final solution response may be a constant in the experiment which may, for example, be concerned with the effects of instructions on problem solving. The influence of past learning may be studied in such a situation by holding other characteristics of the anagrams and word solutions constant while varying Thorndike-Lorge frequencies of the solutions, thereby varying the extent of prior exposure to the solution. Instructions may be varied by specifying in vary-

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ing degree the response class to which the solutions belong. The anagram may be clearly printed on an index card and visible to the subject during his problem solving efforts. However, the anagram may also be exposed tachistoscopically for brief intervals, or the clarity of the type may be systematically varied. The three initial conditions of instructions, word frequency, and clarity of type, and the consequent frequency of correct solutions, may be used to define, respectively, the influence of motivation, learning, and perception upon the disposition called thinking. In a factorial design it would be possible to obtain an estimate of their independent effects upon the final response chain representing the solution. In such experimental situations it is possible to isolate the effects of different variables, including motivation, upon thinking. In other situations it is not possible, very often because there is no independent experimental operation available for manipulating the variable in question. The solution to this problem is simple. Such experimental situations should not be used. They do not represent potentially fruitful approaches to serious experimental work on thinking.

Motivational Concepts

With respect to motivation per se we may be brief. The most extensive development of experimentally grounded motivational concepts is undoubtedly the S-R behavior theory formulations (Hull, 1943; Spence, 1956, 1960). Since there are many excellent discussions of the relevant motivational concepts, they need not be indicated in detail here (Brown, 1961; Farber, 1955).

These motivational concepts may be classified as either associative or

nonassociative in nature in terms of their effects upon behavior. Effective drive state (*D*) in its energizing role is the principal nonassociative variable. It represents the summation of needs present at the moment, including the frustration induced drive of the anticipatory goal response (*K*). Other motivational variables are wholly or in part associative in nature. The drive stimulus is an associative variable. Differential performance based upon characteristically different internal cues is a consequence of associative learning (Hull, 1933; Leeper, 1935). The anticipatory goal response has both associative and nonassociative functions. Its response produced stimulus, s_G , may direct behavior by serving as a source of associative strength for certain responses and not others. In this manner it has a steering effect, evoking specific responses culminating in commerce with a particular goal. Since arousal of the anticipatory goal response and nonattainment of the goal is assumed to be a source of frustration drive which contributes to the effective drive state, thwarting or blocking of goal responses or conflict among responses preventing goal attainment may lead to an increase in the energizing effects of drive (*D*) (Amsel, 1958; Berlyne, 1960; Brown, 1961; Hull, 1952; Maltzman, 1952; Marx, 1956; Seward, 1950; Spence, 1960).

Another distinction of particular importance is between process and state variables. A state variable refers to a relatively permanent or persistent disposition of the organism where the antecedent conditions are previous interactions of the organism and its environment (Maltzman, 1952; Spence, 1948). Learning is a state variable, so also is a hunger need.

Process variables "represent, not states, but hypothetical, nonobservable responses, implicit processes, occurring in the individual" (Spence, 1948, p. 76). A further characteristic of process or response produced needs are that they change relatively rapidly as a function of time since their inception. They are liable rather than persistent dispositions to behave in a particular manner. Frustration or conflict induced motives are of this kind as well as determining tendencies or *Aufgabe* and other kinds of verbal response produced stimuli. These process needs represent by far the most important sources of motivation for thinking.

Associative Factors and Directed Thinking

If the simplifying assumption of treating thinking as involving changes within and between habit family hierarchies is adopted, then much of the S-R discussion of motivation, particularly the Hull-Spence formulation, is immediately applicable to the problem of motivation and thinking.

The importance of the distinction between associative and nonassociative variables may become more apparent if the problems of the direction of thinking are stated as formulated by Humphrey (1940). He has characterized some of the problems which require explanation as follows:

1. What determines the orderly succession of thoughts.
2. What energizes the succession of thoughts.
3. What keeps the successive thoughts relevant to the problem at hand.

Humphrey's survey of theories of thinking circa 1940 indicated that each had difficulties in accounting for

these phenomena. However, at that time characterizations of complex behavior in S-R terms were already available. A decade had passed since Hull's (1930) theoretical analysis of simple trial-and-error learning was published. There is a striking similarity in Hull's statement of the problems posed by trial-and-error learning that demand explanation and those of directed thinking.

1. What principle determines the order of appearance of the different acts.

2. What determines the organism's persistence in responding even after repeated failures.

3. What principle limits the range of reactions which an organism will make to a problem.

Hull's interpretation of the above characteristics of trial-and-error behavior was as follows:

1. The act is evoked at any given state of the trial-and-error process which is strongest at the moment.

2. The organism persists despite failure because the stimulus situation which evokes the acts itself persists.

3. The range of reactions which may be evoked by a given problem situation is limited to the reactions which have become conditioned during the life of the organism to one or another stimulus components of that situation.

Hull in later years specified additional internal sources of persistent stimulation responsible for the persistence of trial-and-error behavior. These were the drive stimulus, S_D , and the s_G , stimulation produced by the anticipatory goal response. Still later he introduced the concept of drive state, D , energizing the habits present at a given moment.

It is significant that at this informal descriptive level all of the characteristics of directed thinking presented by

Humphrey may be accounted for in terms of Hull's theory of trial-and-error behavior. It should be further noted that all of the characteristics of directed or motivated thinking cited are accountable for in terms of associative variables. The question intruding itself, of course, is whether in fact it is necessary to hypothesize an effective drive state energizing thinking. Even if experimental evidence suggests that such a concept is necessary, much of what is considered motivated thinking can be accounted for in the usual associative terms without invoking uniquely different motivational or drive concepts. Excellent analyses of this problem in different contexts have already been presented by Brown (1961), Farber (1955), and Postman (1953), among others. Farber (1955), for example, has shown how many studies of motivation in imaginative productions such as achievement motivation can be adequately explained in strictly associative terms.

Despite the marked similarity between the characteristics of directed thinking and simple trial-and-error learning, it is apparent that there are differences between the two. Humphrey (1940) has not exhaustively described the characteristics of directed thinking. For one thing, reactions to one or another component of a problem situation may occur which have never been conditioned to them. Shifts in the entire direction of thinking may occur in an unchanging problem situation. There may be no external stimulus situation constituting a problem at the moment a given direction of thinking occurs, or the problem situation is an unrelated one. But each of these additional characteristics of directed thinking may be accounted for in associative terms. Of particular importance in

this connection are the concepts of conditioned generalization, compound habit families, and response produced cues. The question still remaining, however, is whether there are any characteristics of motivated thinking that require the introduction of nonassociative constructs. We think that there are. Although currently available experimental evidence is exceedingly meager, we would assume that nonassociative variables of the process need variety which are a consequence of verbal response produced stimulation are necessary for an adequate account of certain characteristics of directed thinking.

Conflict of incompatible response tendencies as a source of motivation or drive has repeatedly been of interest in the recent history of psychology. A number of different formulations of conflict induced drives have been proposed by contemporary theorists (Amsel, 1958; Berlyne, 1960; Brown, 1961; Marx, 1956; Seward, 1950; Spence, 1960), and some such conception was a basic one in the historical school of functional psychology. Dewey (1895) formulated a conflict theory of emotions, and the notion that thinking arises when there is a thwarting of habitual responses (Dewey, 1933) suggests an intimate relation between thinking and drive. Bartlett (1925) has explicitly considered the interdependence of the two variables from a consciousness centered point of view. Berlyne (1960) has recently provided the most extensive analysis of conflict in relation to thinking. With characteristic acuteness he has analyzed many different situations in terms of his conceptions of conflict and has integrated much of the recent research on curiosity and exploratory behavior in this country and the

work on the orienting reflex in Russia. Unfortunately at the present time his ingenious formulations are largely speculations in the sense that they go considerably beyond the experimental data. There is a striking lack of unambiguous data on the influence of conflict induced drives upon thinking.

Research that is the closest approximation to this problem are the studies of the effects of frustration induced by insoluble problems on subsequent problem solving behavior. The hypothesis is that arousal of an anticipatory goal response, verbalization of a goal or problem solution (Maltzman, 1955) induces something akin to frustration drive and is a source of reinforcement when a solution is attained. This terminates the sequence of thinking.

Frustration Induced Drive and Thinking

Although drive has an energizing function, this is not to say that it necessarily is facilitating. Since drive (*D*) indiscriminately multiplies all or most habits present at the moment, successful problem solving behavior may be facilitated or inhibited depending upon the nature of the habit-family hierarchies involved. To conduct experimental studies of drive and thinking, it is necessary that there be independent control of the antecedent conditions related to the hypothetical variables of drive and habit-family hierarchy. Failure on insoluble problems meets the requirements for conflict induced drive, and the so-called mental set or Einstellung experiment permits the experimental manipulation of the habit-family hierarchy.

A common procedure employing Luchin's (1942) water jar problem is to present subjects with a series of

problems all solvable by means of the so-called long solution. As a result of training, this solution acquires the greatest habit strength. When a problem is presented in which an alternative short solution is possible the dominant tendency as the result of training is to give the long solution. If the effective drive state is increased by frustration, the absolute difference in reaction potential for the long and short solutions will increase. There will be a greater tendency for the long solution to occur as a consequence. Cowen (1952) and Pally (1955) have employed essentially this procedure obtaining results in accord with the drive interpretation.

In the absence of a clearly established habit-family hierarchy, Rhine (1957) found that failure on pretest anagrams produced a significant decrement in test anagrams. There was a tendency for a greater frustration decrement when failure and test anagrams belonged to the same response class than when they belonged to different response classes. These results suggest that associative variables at least in part are involved in failure produced interference with problem solving, and would make a drive interpretation superfluous in the absence of additional evidence of facilitation effects.

Solly and Stagner (1956) have found that solution times for an anagram increased significantly as a function of prior failure on insoluble anagrams. Again, an associative as well as a nonassociative or drive interpretation may be given these results. Evidence supporting the drive notion, although by no means definitive, is the fact that measures of palmer sweating taken before and after the experiment revealed that the failure group showed a signifi-

cant increase in palmer sweating as compared to a control group.

Glasser (1958) also employed anagrams as test materials in a study of the effects of failure. He frustrated subjects with dissimilar stimulus materials, the water jar problem. Reliable evidence was obtained for facilitation following failure when the previously established dominant anagram set remained dominant. He likewise found that the galvanic skin response increased as a consequence of problem solving failure.

Although these studies of failure frustration may be given a drive interpretation, they do not unambiguously support such a conception. As Child and Waterhouse (1953) have indicated, individuals may learn to perform differentially in the presence of frustration produced cues. Frustration may be learned as a cue for more effective thinking. Such an associative interpretation of the consequences of failure in, for example, Cowen's (1952) and in Glasser's (1958) studies must be ad hoc since the specific conditions under which facilitation or interference occurs on an associative basis are not stated. A drive interpretation of frustration as formulated within the Hull-Spence theory of behavior more explicitly states the conditions under which these opposed effects of frustration are to be expected. But this as well as other S-R theories also assumes that drive stimuli may serve as internal cues. Specification of the precise conditions under which frustration induced cues are conditioned to different behavior patterns and the conditions under which these behaviors are reinstated is a serious problem for behavior theory generally. A clear demonstration of frustration drive energizing problem solving would require a factorial de-

sign with dominant correct and incorrect mental sets, habit-family hierarchies, evoked following conditions of failure and nonfailure. It should be possible to demonstrate for the same subjects that frustration may result in superior or inferior problem solving depending upon the interaction of drive and dominant habit structures present at the moment. Glasser (1958) designed his experiment in this manner, but could not perform an adequate test because he failed to obtain a dominant set for the incorrect solutions. Maltzman, Fox, and Morrisett (1953) obtained facilitation and interference with manifest anxiety as an irrelevant drive, but their experiment is open to the criticism that the opposed effects were obtained with different groups of subjects and with two different kinds of problems. That the predicted effects of frustration induced drive on problem solving may be obtained is indicated by the unambiguous results of Castenada and Lipsitt (1959) employing a simple motor learning situation.

Another relevant condition when considering the effects of frustration drive on problem solving is the similarity between the failure and test situations. Whether or not the correct habit family is dominant, decrements in performance may occur if subjects are frustrated with highly similar problems whereas facilitation would only occur when failure is induced with different materials. Under the latter condition competing responses are associated with the failure and not the test situation. Under such dissimilar stimulus conditions only the nonspecific drive state could influence performance in the test situation. Support for this hypothesis stems from animal studies of the effects of shock induced emo-

tionality which have shown that rats shocked in one situation show a significant increase in water intake when tested in a different stimulus situation (Amsel & Maltzman, 1950). When shocked and tested in the same situation they show a decrement in consummatory behavior (Amsel & Cole, 1953).

Murdock (1952) has conducted a study of the effects of failure which is not concerned with frustration drive as an energizer but as a source of reinforcement for avoidance learning. Employing a mediated generalization procedure he demonstrated that failure associated with particular responses retarded subsequent perceptual motor learning where these responses could serve as mediators of new learning. Avoidance of the mediating responses associated with failure presumably was reinforcing. Extension of the analysis of avoidance of implicit verbal responses may serve as the basis for so-called suppression and repression in thinking (Dollard & Miller, 1950).

In contrast to the previous experiments, Kendler, Kendler, Pliskoff, and D'Amato (1958) have conducted an ingenious experiment suggesting the energizing role of a positive incentive. The problem solving situation that they employed required the assembly of previously isolated habit segments on the part of preschool children. Such reasoning was found to be dependent upon the presence of a positive incentive as well as reinforcement of the separate habit segments.

Determining Tendency and Process Needs

Historically the most prominent concept employed in the analysis of directed thinking has been mental set, a term used here initially to include

terms such as *Aufgabe*, determining tendency, and goal idea, etc. The kind of behavior usually described as a manifestation of mental set is an increased frequency of occurrence of a particular class of responses. A concomitant behavioral change often used as a criterion of mental set is a decreased frequency of occurrence of a new class of responses. It is obvious that such behavioral changes can be produced by a variety of antecedent conditions such as those commonly related to learning, motivation, generalization, in other words all of the variables known to influence behavior.

Two kinds of set may be distinguished on the basis of their antecedent conditions and functional relations to consequent behavior changes. These correspond to state and process variables. The learning of response classes or the acquisition of habit strength by a class of stimuli for the elicitation of a particular class of responses is of the state variable kind. A second kind of set that has been studied employs task instructions which produce a change in the compound habit-family hierarchy by increasing the reaction potential of a class of responses through arousal of their anticipatory goal response.

Sets developed through training are based on the growth of habit strength, and therefore develop relatively slowly and have a degree of permanence. In contrast, sets induced by instructions are labile, are aroused rapidly and need not persist (Maltzman, Eisman, Brooks, & Smith, 1956, p. 420).

The latter kind of set or determining tendency possesses the characteristics of a process need. Arousal of the anticipatory goal response presumably produces an immediate increase in reaction potential for all its associated responses, the habit-family hier-

archy of which it is a member. Such an effect occurs because the goal response enters into a multiplicative relationship with habit strength in the determination of reaction potential (Maltzman, 1955). It constitutes a major source of drive in the energizing sense, and directs thinking by energizing primarily those responses which constitute a common habit-family hierarchy, a common core of intraverbal associations. Arousal of the anticipatory goal response and continued nonattainment of the goal, frustration, produces further increases in drive. Frustration may be the sole antecedent source of drive, since in problem solving by definition a correct response does not initially occur. Solution of a problem terminates the goal response, reduces frustration, and is thereby reinforcing. Whether all thinking is goal directed or not we do not know. In part this question is a matter of definition, how "goal" is specified.

Experiments demonstrating that instructions may produce a rapid increase in the probability of a response class have been reported by many investigators (e.g., Hunter, 1956; Maltzman et al., 1956; Maltzman & Morrisett, 1953; Rees & Israel, 1935) since the Wurzburg School (Humphrey, 1951; Titchener, 1909). In problem solving studies, it has repeatedly been demonstrated that merely instructing subjects at the start of a session that the anagram solutions all belong to a particular class, such as nature words, produces reliable evidence of facilitation. It may be argued that the effect is obtained because the instructions reduce the range of appropriate responses by eliminating many incorrect solutions before they are attempted. This is quite correct, but the basis for the effect is the one

previously stated. A particular class of solution responses is immediately increased in reaction potential, becomes dominant in the compound hierarchy of habit families elicitable by the task.

Determining tendencies as a form of process need are often instigated by the instructions or verbal response produced cues of the experimenter. Another pervasive form of process need has gone under a variety of different labels such as prompts, hints, recency, priming, etc. (Cofer, 1960; Maltzman & Simon, 1959; Osgood, 1957; Skinner, 1957; Storms, 1958). Judson and Cofer (1956) have demonstrated the effect employing the Four-Word Problem Test. Two of the words in each item of this test are ambiguous in that they are members of two different relevant response classes or habit-family hierarchies while the other words are unambiguously a member of only one of the two relevant classes. The subjects' task is to indicate which word is unrelated to the other three in the item. An illustrative item is the following: "Add" "Subtract" "Multiply" "Increase." Add and Multiply are ambiguous words in that they belong to both the class of arithmetic operations and of growth functions. Subtract and Increase are unambiguous in that the former belongs only to the relevant class of arithmetic operations while the latter belongs only to the relevant class of growth.

The hypothesis tested was that whichever of the unambiguous words occurred in an item first would activate or differentially increase the reaction tendency for that class of responses, resulting in the exclusion of the second unambiguous item as unrelated to the other three words. Thus if Subtract appeared in the item before Increase, the hierarchy

of arithmetic operations would be differentially facilitated and Increase would be eliminated as irrelevant. If Increase occurred first the response class to which it belongs would receive an increment in reaction potential so that Subtract would be eliminated. The word order of the unambiguous terms was systematically varied. Results obtained in two studies indicated that the unambiguous word which appears first is less frequently eliminated than the second regardless of the particular word involved or the particular position in the item. These results indicate that the occurrence of a single verbal response increases the probability of occurrence or availability of a class of associated responses or habit family.

Another problem situation reported by Judson, Cofer, and Gelfand (1956) yielded data again suggesting that the occurrence of a given response increases the likelihood of occurrence of associated responses. Here, reinforcement in a verbal problem of one member of a word-association hierarchy increased the frequency of selection of associations from that same hierarchy when they appeared individually in different verbal problems.

Another study (Maltzman & Simon, 1959) has shown that the uncommonness of responses to a word-association list increases as a function of the recency of association to different stimulus words.

Systematic research on the parameters of response-produced process needs is urgently needed, and represents one of the most intriguing problems in the area of thinking. It also represents what appears to be a problem largely peculiar to verbal behavior. Considerably more research is needed before these verbal response-instigated changes can securely be

characterized as involving changes in drive state (*D*). Particularly pertinent in this respect would be systematic research designed to determine whether or not decrements in process needs contribute to the acquisition of habit strength, whether they may serve as the basis for learning.

One last source of verbal response-instigated needs must be mentioned, one which is of considerable practical and theoretical significance, but which has received much less study in this country than in the Soviet Union. This is the changes in drive states that may be induced as a consequence of the instigation of physiological changes conditioned to verbal response-produced stimuli. Razran's (1961) review of research in Russia, as well as translations of such work, attest to the profound influence conditioning may have upon physiological functioning. Of particular pertinence to our present discussion is the role of verbal response-produced stimuli or the second signal system as complex conditioned stimuli for physiological responses. For example, Platonov (1959) reports verbally conditioned pain reactions as well as analgesia, the latter condition known for many years to be produced under hypnosis. But as Platonov convincingly argues, hypnosis is a form of verbal conditioning. Among other physiological changes, Platonov reports that the leucocyte count may be increased as well as reports of feelings of hunger by suggesting to the subject that he is hungry, by employing words as conditioned stimuli for these physiological changes. Appropriate verbal suggestion likewise produced a drop in the leucocyte count. Experimental studies are clearly needed in which an attempt is made to verify these findings and to determine whether performance under different

conditions such as classical and operant conditioning, simple verbal learning, and problem solving, are affected in the predicted manner if changes in drive are in fact induced.

The area of thinking is of central importance in the study of human behavior. There has been an obvious reawakening of interest in problems in

this area during the past decade and some signs of real progress. Within this relatively neglected area motivation has been a most neglected problem. This state of affairs will surely change. But it will change for the better only if there is systematic experimental research integrated with rigorous behavior theory.

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LINGUISTIC STRUCTURE AS RELATED TO CONCEPT FORMATION AND TO CONCEPT CONTENT

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Motivation, in current psychological theory, has two general effects. To motivate is to arouse; to cause the organism to attend more closely to the environment. To motivate is also to influence the direction of the organism's attention; to increase the probability that the organism will respond to one class of stimuli rather than other classes of stimuli.

It is the directional aspect of motivation as it is related to concept formation that shall be discussed in this paper. It is generally known that abstract concepts are more difficult to learn than are concrete ones. One means of explaining this empirical fact is to invoke various developmental theories. The purpose of this paper is to discuss these theories, to examine the data supporting these theories, and to offer an alternative explanation of observed differences in the adequacy of attainment of concrete and of abstract concepts. This explanation is based on certain ideas of Benjamin Lee Whorf (1956) which form the basis for a theory of linguistic determinism.

The formation of a concept involves the development of awareness of characteristics common to certain objects, attributes, or ideas, so that those objects, attributes, or ideas having elements in common may be grouped into a separate category or class. Vinacke (1952) defines concept formation as involving

processes of perception and learning by means of which the individual develops an organized and coherent relation to the outside world. The consequences of these processes is the establishment of concepts, the cognitive

structures which link the individual's present perceptions and learning to his previous experience (p. 98).

Vinacke continues, following the above quotation, by saying that we must distinguish between the process of concept formation and the contents of the concepts formed, a distinction that I will try to maintain in this paper.

A number of psychologists, such as Piaget (1950, 1951), Goldstein and Scheerer (1941), and Werner (1948) have constructed developmental theories bearing on the problems of concept formation and on "mental structure." These theories differ in various ways, but have a number of points in common. The child is said to develop (once past a strictly sensory-motor stage) from a concrete-perceptual to an abstract-conceptual level of thought. The change from concreteness to abstractness is a relatively sudden, saltatory two-stage process, with mature abstractness in thought first occurring at about age 12, as a result of unspecified changes in mental structure.

Since definitions of concreteness and of abstractness vary, the definitions used herein are offered below. The formation of concrete concepts involves an organization of experience in which the grouping of perceptual elements of the stimulus situation is sufficient for adequate categorization. The formation of abstract concepts involves some form of classification of experience for which sensory experience is insufficient, in itself, as a basis for accurate categorization. Further organization,

beyond the purely perceptual level, is required before adequate conceptualization can occur. Concrete concepts are generally horizontal; abstract hierarchical or vertical in nature. Such concepts as size, color, or shape are examples of concrete concepts, while such concepts as justice, causality, and life (this last referred to as animism when incorrect in concept content) are examples of abstract concepts within this definition. There is certainly more to concreteness and abstractness than this, but this definition is adequate in the sense that all of the concrete-abstract theorists cited would probably accept it as a minimal definition.

It may be muddying the waters at this point, but I would like to suggest that there are three, not two, levels of concepts along this concrete-abstract dimension. The first, most concrete level is that which refers to the attributes of objects, such as size, shape, or color. A second stage, while still concrete-perceptual, involves those stimulus characteristics of objects that are more dependent on external frames of reference, such as quantity and position. The third stage, in this division, would be, as in previous categorizations, the abstract-conceptual level for which purely perceptual organization is insufficient.

From the developmental views of Piaget, Goldstein, Scheerer, and Werner, we would expect concrete concepts to be formed earlier than abstract ones. Since the development of abstract concepts depends on a qualitative, agebound, presumably physiological change in mental structure, we would expect that:

1. No abstract concepts will be formed by children before approximately age 12.

2. To the degree that concepts are equally abstract, they should be

formed at about the same time by an individual.

3. Individuals within a culture should not vary markedly in age of concept attainment.

4. There should be little cross cultural variation in age of concept attainment between members of various nonprimitive societies. I include the phrase "nonprimitive," since for some theorists, such as Werner, all primitive people, even when adults, are "concrete." Perhaps, as Roger Brown (1958, p. 297) suggests, we have the unfortunate habit of labeling the thoughts of others as concrete, if the thoughts follow unfamiliar lines, while reserving the accolade of abstract for our own brand of thinking.

Let us examine the development of some abstract concepts to determine whether they fulfill these expectations.

Piaget (1929) investigated animism in children, attempting to measure developmental changes in the kinds of objects that subjects believed to be alive. Children first believed anything active to be alive; e.g., a telephone is alive, at least when it is ringing. Later in development, life is attributed only to objects that move. Still later, life is attributed to objects that move without visible external stimulation, such as the sun or the wind. Finally, consciousness or life is restricted to plants and animals by about age 12. Most investigators (e.g., Russell, 1940; Russell & Dennis, 1939) have found these stages to exist and have found some age progression, but have also shown considerable variation in the level of explanation used at any age level. Dennis (1942) found his daughter (IQ 150) passed through Piaget's four stages, reaching an adult level of understanding of the concept, "life" at 6 years 2 months.

Dennis (1953) also studied college students and found that a considerable proportion of them retained some animistic beliefs. In one of the relatively few cross cultural studies (as opposed to observations, of which there are a great number) of animism, Huang and Lee (1945) found almost no evidence of animism in Chinese children. While the results depend, to a considerable extent, on the procedure followed, intra- and intercultural studies suggest no universal, saltatory age change in concept content.

An aspect of a concept of justice, or of moral judgment, is moral realism. Moral realism is the tendency to judge acts exclusively in terms of consequence without regard for motives. Maine (1861) says that the history of jurisprudence is that of a gradually increasing concern for motives. We have not yet fully evolved to what Maine (1861) or Piaget (1932) would call complete moral maturity in this area, even as adults. Certain age changes in moral judgment fit the concrete-abstract explanation of changing mental life. Piaget (1932) finds young children are morally realistic, while older children are not. This is a result, in part, of the concreteness of children's thought processes. The change toward a concern for motives is complete, according to Piaget, at about age 12. Yet very young children in the Chinese culture have already advanced beyond this stage (Liu, 1950). The advancement of Chinese children beyond Caucasians of the same age is a result, says Liu, of a culture and a philosophy that require acts to be considered in terms of cause or motive. On the other hand, high school children in our culture still show many evidences of moral realism (Johnson, 1962).

Causality is another concept studied by Piaget (1930) and others. The child moves from magical explanations in terms of some animistic volition of the object (e.g., the candle in the airtight jar goes out because it is tired) through a large number of types of explanation to a final adult conception of causality. Here again there is evidence that age progression in the achievement of accurate concept content is not as neat as Piaget suggests, since almost all forms of explanation are present among children by about age 8 (Deutsche, 1937). On the other hand, we, as adults, seem to be by no means "out of the woods" in our handling of causality.

I would like to mention one more abstract concept, that of "time." While time has concrete referents, such as day, night, and moon, many more distinctions, only some of them tied to clock time, are possible. Ames (1946) reports that by about age 8 the superior child's understanding of the time concept is essentially a mature one.

I have presented data concerning four concepts that usually are considered to be abstract ones. The discussion has centered largely on concepts first studied by Piaget, since Piaget's variety of concrete-abstract theory is the one for which the most evidence is presented. To the extent that other theorists would accept my definition of abstractness, the results obtained in testing Piaget's ideas should also hold for their theories.

Presumably, from a concrete-abstract theoretical framework, abstract concepts should be formed only after about age 12. Looking at research on children, it seems pretty clear that what are usually termed abstract concepts are formed rather early, but with inaccurate or inadequate content based on concrete perceptual

elements of the stimulus situation that are not central to the concept, but instead (as in the case of movement being confused with life) showing a fortuitous relation to the concept. The 5-year-old has a concept of life; it is just that we believe him wrong in concept content when he attributes life to a moving locomotive.

Abstract concepts may be generally formed so that concept content is accurate during the period when a child is said to be tied to concrete-perceptual categorizations of events, as in the case of the concept of time achieved by superior 8-year-olds. Individuals within a culture vary greatly in the age at which adequate concept content is attained, as for example, in concepts of causality and animism. Further, considerable variation exists between subjects from different cultures in the age at which a given level of concept content is attained. These data do not seem to argue in favor of a theory of concept formation based on apparently innate saltatory changes in mental structure that should occur at approximately the same age for all individuals in nonprimitive cultures.

I do not believe that the developmental theorists whom I have discussed have proven their position to be correct. In fact, the evidence appears to be against their point of view.

I would now like to present another possible explanation for the fact that the content of concrete concepts is generally easier to learn than the content of abstract concepts. In this connection I shall discuss concept formation and concept content from what is usually referred to as a Whorfian point of view. This position (Whorf, 1956) is that higher levels of thinking are dependent on language

with the structure of the language influencing the manner in which one organizes and understands his environment. Within this framework, the probability that an individual develops a concept depends on the number of available words bearing on the concept. If no words or only a few words are available to denote a possible dimension of experience, the probability is increased that these objects or events will be categorized in some other fashion.

However, the availability of a very large number of words to denote objects falling within one potential class or category also appears to decrease the probability of forming a higher level concept encompassing the whole class or category, as shown, for example, in the Lapps having no generic name for snow (Werner, 1948) and the Bakairi of Brazil having many names for kinds of parrots but no name for the class that we would call "parrot" (Brown, 1958). The relation between the number of words available to denote objects that are similar along some dimension and the probability of the development of a concept encompassing this dimension of experience may, then, be curvilinear. The concept is somewhat more likely to be formed within the broad range from very few to very many words.

It seems likely that it is in the area of concept content that a Whorfian view is more meaningful. From this viewpoint, one would say that discriminatory ability depends, to a degree, on the availability of symbols. The Eskimo, with his 40 names for snow, is more likely to try to discriminate between varieties and presumably is also more accurate in discriminatory ability. He has a more accurate notion of concept content, whether as a result of the greater

availability of symbols or of increased practice at discrimination.

The "goodness" of a concept, in terms of allowing accurate categorization of objects or events, would depend on two things. The first, already mentioned, is the number of words available for categorization of objects into any particular higher level concept. The second is the exclusiveness of the words used to denote the essential elements of the concept. For example, a characteristic of automobiles is movement. Movement is not, however, a good term to use as a verbal base for the concept automobile. Automobiles are still automobiles even when not moving. Many other objects move.

The adequacy of a concept, in terms of content, would, from a linguistically relativistic point of view, depend on the number of words available to describe elements of the concept and on the exclusiveness of these words. I have some data I wish to present, regarding the number and the exclusiveness of English words that are related to various concrete and abstract concepts. This study was modeled after that of Brown and Lenneberg (1954). In the Brown and Lenneberg study 10 judges were presented with the entire series of Munsell colors at the highest level of saturation and asked to map the color regions referred to by the color names of red, orange, yellow, green, blue, purple, pink, and brown and also to choose one color chip in each of these color areas that best represented that color. A high amount of agreement was shown between judges in mapping color areas and in choosing "ideal" chips. These 8 ideal chips plus 16 other chips that were removed, to varying degrees, from the central or ideal colors of the various color areas were presented to 24 sub-

jects who were asked to name each of the 24 colors as it was presented. Five measures were taken during color naming. These were: (a) average syllable length of the names given to each color, (b) average number of words used to name each color, (c) latency of color naming, (d) amount of agreement between subjects in color naming, and (e) test-retest reliability of names given each color. All of these measures were positively correlated with one another, forming a *g* factor called codability. The amount of agreement between subjects proved to be the best single measure of codability. The chips of highest codability were the ideal instances from within the eight color regions.

Brown (1958) says:

It is not so clear how to plot a category like chair because we are not sure of the defining attributes of this category. . . . It is proposed that central stimuli will be highly codable and peripheral stimuli less codable. These thoughts are offered as blueprints for a set of psychological laws relating category codability to category availability and stimuli codability to category centrality (p. 241).

From this theoretical framework and from the above results, we would expect that the number of verbal symbols available to describe attributes of a stimulus or the position of a stimulus along the dimension or experience denoted by a concept would be positively correlated with the codability and discriminability of stimuli along this dimension. The amount of agreement among subjects regarding symbols available in any area of conceptualization should indicate the amount of interpersonal agreement as to the relevant components of the concept. For example, given the concept automobile, we could produce many terms describing this concept, ranging from words like "Ford" and "4-door" through "MG"

and "saloon model" and down to infrequent responses like "Essex" and "phaeton." Within the Whorfian framework, the more relevant words we could find, the more adequate the understanding of the concept. The more agreement between us in terms, the more likely we will be talking of the same thing when we talk of automobiles in a generic sense. The exclusiveness of the words used would also be indicative of the adequacy of concept content. If to "automobile" we responded with "truck," "tractor," "weapons carrier," and terms of this sort, we would be overinclusive and incorrect in our concept.

It is possible to measure abstract and concrete concepts with regard to their codability and the adequacy of concept content. I chose seven concepts to examine. Three of them, life, cause, and time, are concepts that should, within Goldstein's definition, require abstract conceptual thought for adequate development.

Twenty-seven subjects, all college students, were presented with the following instructions:

In our lives we must learn many concepts, some of which have to do with the grouping or categorization of concrete objects, others with such abstract qualities as justice. A number of concepts will be presented to you on the next two pages. Following each concept are two words associated with it, indicating how we might describe an object or event in terms of that concept. Your task is

to write other terms for each of the concepts. You have three minutes for each concept. I will let you know when one minute is up, two minutes are up, and when the three minutes have been concluded, and will tell you when to begin and when to finish for each concept.

The subjects were then presented with the seven concepts arranged on two sheets of paper, in random order with the order varying between subjects. Each concept was followed by two words that might be used to describe an object or event in terms of the concept; e.g., shape—triangular, blunt; life—animate, alive.

The responses of the 27 subjects were placed on alphabetized tally sheets. The total number of different words used to describe attributes of stimuli or the position of stimuli along the dimensions of experience denoted by the various concepts were obtained. It had been hypothesized that the amount of agreement among subjects regarding symbols available in any area of conceptualization would indicate the amount of agreement between subjects in concept content. It was assumed, in this study, that the number of symbols attached to a conceptual dimension by more than 10% of the subjects was a crude but useful measure of degree of agreement between subjects in concept content. The results are presented in Table 1.

TABLE 1
SYMBOLS ATTACHED TO VARIOUS CONCEPTS

Color	Shape	Position	Quantity	Time	Cause	Life
Number of Different Descriptive Words						
186	92	247	153	177	192	173
Number of Descriptive Words Used by Three or More Subjects						
60	37	66	45	37	9	17

TABLE 1—Continued

Percentage of Descriptive Words Used by Three or More Subjects						
32	41	27	29	21	5	10
Words Used by Three or More Subjects						
Aqua	Acute	Among	All	Annual	Beginning	Active
Amber	Big	Around	Alot	Autumn	Build	Breathing
Auburn	Circle	Away	Amount	Biennial	Create	Conscious
Blue	Cube	About	Any	Century	Commence	Creative
Brown	Circular	Amid	Bunch	Day	Do	Energy using
Beige	Diamond	After	Bushel	Decade	Push	Existing
Bright	Flat	Above	Couple	Eternity	Reason	Feeling
Blue-green	Fat	At	Either	Evening	Start	Generate
Baby blue	Heart	Across	Each	Forever	Stimulate	Growing
Chartreuse	Hexagon	Below	Every	Future		Has emotions
Chinese red	Irregular	Beside	Few	Hour		Learning
Charcoal grey	Jagged	Beneath	Foot	Infinity		Lively
Coral	Long	Beyond	Gallon	Long		Living
Cinnamon	Large	Back of	Great	Light year		Moving
Chocolate	Narrow	Betwixt	Gross	Later		Not dead
Cream	Oblong	Before	Group	Month		Purpose
Dull	Oval	Behind	Hundred	Morning		Thinking
Dark	Octagon	By	Inch	Millenium		
Flesh	Parallel	Bottom	Little	Moon		
Green	Pointed	Close	Large	Never		
Gold	Pear	Central	Lots	Night		
Grey	Pyramid	Down	Less	Now		
Hazel	Pentagon	Diagonal	More	Past		
Indigo	Polygon	East	Most	Present		
Ivory	Round	First	Much	Second		
Lime	Rectangular	Far	Million	Sometime		
Lemon	Rough	Front of	None	Short		
Light	Square	High	Numerous	Semester		
Lavender	Short	Horizontal	Number	Spring		
Lilac	Small	Higher	Neither	Summer		
Magenta	Sharp	Here	One	Today		
Mauve	Smooth	In	Ounce	Tomorrow		
Maize	Sphere	Inside	Sound	Week		
Maroon	Star	Into	Peck	When		
Navy	Thin	Juxtaposed	Pint	Winter		
Orange	Trapezoid	Last	Quart	Year		
Olive	Wide	Low	Some	Yesterday		
Pink		Left	Several			
Purple		Lower	Small			
Platinum		Middle	Single			
Red		Next to	Two			
Royal blue		Near	Three			
Rose		North	Tens			
Red-orange		On	Thousands			
Rust		Over	Yard			
Sky-blue		On top of				
Silver		Out				
Scarlet		Outside				
Sea-green		Second				
Straw		Straight				
Steel blue		South				

TABLE 1—Continued

Words Used by Three or More Subjects	
Turquoise	Slanted
Tan	Separated
Toast	Side
Violet	Through
White	There
Warm	Top
Wine	Third
Yellow	Together
Yellow-green	Touching
	Under
	Underneath
	Up
	Upper
	Upon
	West

While the total number of words used by this sample of subjects to describe the various concepts did not vary in a predictable fashion between concepts, the proportion of common, nonidiosyncratic responses did vary in the predicted direction. Agreement between subjects was greatest for the two concepts, color and shape, that describe concrete attributes of objects themselves. Agreement was somewhat less for subject's responses describing position and quantity, concrete attributes that require viewing the object with reference to other objects. Agreement in concept content was still less for the three abstract concepts. Time, the only abstract concept for which adequate concept content is attained rather early in this culture, showed more intersubject agreement than did the responses to the other two abstract concepts.

We find some confusion in concept content even in concrete concepts, such as shape, where common responses include the words "big" and "small," which are actually descriptions of size. It is in the area of life, however, where inaccurate concept

content is most noticeable. Attributes of life include such words as growing and moving, two terms that are also used to describe nonlife. Of all responses made, "moving" was made by the most subjects, paralleling the obtained responses of young children.

Difficult concepts, in terms of reaching adequate concept content, are clearly concepts for which there are few nonidiosyncratic descriptive terms available, and/or for which common descriptive terms are not accurate in terms of concept content. While this is what would be predicted from a Whorfian framework, it is not proof that a Whorfian explanation is correct, since in this whole area it is difficult to determine what is cause and what is effect. Does a paucity of words cause us to disregard a dimension of experience or does our disregard of a dimension of experience result in our having a very limited number of words to describe it? It should be possible, however, to test the linguistically relativistic position taken in this paper by comparing the language structure of cultures where a given concept is developed ade-

quately at an early age to cultures where the concept is developed adequately at a later age. It should also be possible to test this explanation of concept formation by using small children as subjects and building into their vocabularies a number of nonsense words describing attributes or objects or events not usually distinguished from one another in the English language.

The testability of the linguistically deterministic explanation is probably greater than the testability of the genetic or developmental explanation of concept formation, since the developmental theorists must rest, ultimately, on saltatory changes in the brain connections as an explana-

tion for the changes in conceptualization that they describe.

To summarize, we have examined two explanations of empirical data showing that concrete-perceptual concepts are learned to an adult level of understanding earlier than are abstract concepts. Data do not fully support a developmental explanation. A linguistic relativistic position is supported, but not proved to be correct. If the linguistic relativistic position is correct, then linguistic structure is a motivating force in the thought process, since it causes us to attend to some aspects of the environment more closely and more accurately than to other possible dimensions of experience.

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IMPULSIVE VERSUS REALISTIC THINKING: AN EXAMINATION OF THE DISTINCTION BETWEEN PRIMARY AND SECONDARY PROCESSES IN THOUGHT¹

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Because we so commonly characterize Freudian psychoanalysis as a dynamic psychology, or a developmental psychology, or a psychology that emphasizes conflict or the relief of symptoms, we tend to translate the conceptions of psychoanalysis into an active mode. This tendency sometimes causes us to overlook the fact that psychoanalysis is very largely a *cognitive psychology* concerned primarily with mental representations, with hallucinations and dreams, with memories, their distortions and repression, with attention and inattention. Of course one might say that all psychology was mentalistic when Freud was writing, and that he was really talking about overt behavior and not about symbolic behavior. This I believe to be incorrect: Freud was very much concerned about symbols; his mental representations, condensations, displacements, and the rest are essentially cognitive. It is appropriate for us to consider Freud's views in this symposium, for his psychology was at once a cognitive psychology and a psychology of motivation.

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The basic cleavage in thought, according to Freud, is between two processes, the earlier and more primitive *primary process*, and the later more orderly, rational, and reality-oriented *secondary process*. I wish to examine this distinction to see of what service it might be within general psychology.

The distinction between the illogical and impulsive in thought, on the one hand, and the logical and rational, on the other, is of course a very old one, and is not original with Freud. Every elementary logic course points out the circumstances that lead to fallacious thinking, and these include the *argumentum ad hominem*, and other kinds of argument that permit prejudice to blind judgment. The notion that "the wish is father to the thought" did not begin with Freud. Contemporary writers, too, such as Piaget and Werner, arrive at distinctions between earlier and later modes of thought. Hence some such distinction as that which Freud makes between primary and secondary process is plausible enough.

The question for us to face is not whether this distinction is plausible, but whether there are novel features in Freud's conception that are important, whether the concepts are clear, and whether there are suggestions for empirical work deriving from them.

TWO PROCESSES ACCORDING TO FREUD

The distinction between primary and secondary processes is so perva-

sive in psychoanalysis that it often receives scant mention by psychoanalytic writers who fully accept it. This may be in part because the terms belong to the metapsychology, and the clinical literature of psychoanalysis is commonly not expressed in these terms. The more theoretical discussions of psychoanalytic theory invariably find a central place for these processes. Freud's biographer says: "It was this distinction on which rests Freud's chief claim to fame: even his discovery of the unconscious is subordinate to it" (Jones, 1955, p. 313), and the translator of his *Interpretation of Dreams* says in a footnote: "The distinction between primary and secondary systems, and the hypothesis that psychical functioning operates differently in them, are among the most fundamental of Freud's concepts" (Freud, 1953, p. 601). It is of some interest, therefore, to review the attention that Freud gave these terms, and then to try to assess their meaning for a general psychology of cognition.

Freud introduced the terms in his *Project for a Scientific Psychology*, prepared in 1895, but not published until after his death along with his letters to Fliess. The first mention was in a letter to Fliess dated October 20, 1895, with reference clearly to the *Project* upon which he was then working. The relevant section in the *Project* is entitled "Primary Processes: Sleep and Dreams" (Freud, 1954, pp. 397-404). Here most of the later ideas are anticipated, although at this stage they are couched as a speculative neuronal theory—a theory that at least one competent reviewer finds to be in many ways an anticipation of contemporary developments in neuropsychology (Pribram, 1962).

The next full-scale discussion is in the *Interpretation of Dreams*, with the relevant section entitled "The Primary and Secondary Processes: Repression" (Freud, 1953, pp. 588-611). Freud returned briefly to the problem from time to time thereafter, the most important single paper being "Formulations on the Two Principles of Mental Functioning" (Freud, 1958). Later papers helped to coordinate the two processes with later developments in the theory, such as the new "death instinct" (Freud, 1955) and the new id, ego, superego structures (Freud, 1961).

The most painstaking effort to understand what Freud meant and to cast what he said into the form of conceptual models was made by Rapaport in a series of papers (Rapaport, 1950, 1951a, 1951b, 1957, 1959, 1960; Rapaport & Gill, 1959), all of which bear in one way or another on the problems of motivation in thinking. The main conclusion to which Rapaport came is that there are two kinds of organization of memory which become reflected in the two kinds of thinking: drive-organization² and conceptual-organization, the former representing, of course, primary process, the latter secondary process.

In carrying through the conceptual

² It is not possible in a brief paper to deal with all the puzzling problems that are raised in trying to be at once appreciative and critical of psychoanalysis. In accepting the *drive* concept from psychoanalysis, and coordinating it with what most psychologists mean by drive, we overlook a rigidity within writers on classical psychoanalysis who recognize only two drives (sex and aggression), despite the primitive nature of pain, hunger, thirst, temperature, contact, curiosity, manipulation, and the other candidates for inclusion as drives. Freud did not settle the matter once and for all in 1920 when he proposed the death instinct, which for his followers made aggression a second drive along with sex.

distinction between primary and secondary process, Rapaport deduced from Freud primary models of action, cognition, and affect (indicating their characteristics when primary process is in control) and secondary models of each of these, when the delays of secondary process are introduced (Rapaport, 1959, pp. 71-78). The primary model of *action* is that familiar in the drive-reduction theory of motivation: aroused drive-tension, presence of the incentive and response to it (in psychoanalysis, sucking the mother's breast), followed by drive reduction. The primary model of *cognition* is aroused drive tension in the absence of the incentive, leading to hallucination of the incentive. Finally, the primary model of *affect* substitutes affect discharge for hallucination. Thus the hungry infant may scream instead of hallucinating the breast. All primary models indicate prompt response to the drive that reaches threshold intensity; all secondary models introduce delays. The secondary model of action introduces a derivative drive (similar again to learning theories that study the drive value of familiar paths and the secondary reinforcement value of subgoals). The role of inhibition (in the absence of the goal-object) is also stressed; again something familiar in the learning-theorist discussion of frustration-induced drives (Amsel & Roussel, 1952; Marx, 1956). The secondary model of cognition substitutes for the hallucination of the object a search for it, i.e., ordered thinking. The secondary model of affect substitutes for massive affect discharge a lesser anticipatory discharge that serves instead as a signal; behavior may be released which defends against the more massive affect discharge. There are complexities

within each of these models that this brief summary cannot deal with.

Some of the characteristics of the two processes which we need to examine in relation to a general theory of thinking are the following:

1. Primary process is earlier in time and more primitive than secondary process. This does not mean that it is ever outgrown, however, for primary process functioning is characteristic of the normal adult as well as the infant, e.g., in dreams.

2. When the primary process holds sway, wishing ends in hallucinating; the infant is said to hallucinate the satisfaction of its internal needs when they cannot be gratified at once. Massive affective discharge is an alternative.

3. Primary process is coordinated with the pleasure principle, secondary process with the reality principle.

4. The pleasure principle "reigns unrestrictedly in the id" and the ego endeavors to substitute the reality principle.

5. The formal characteristics of primary and secondary processes differ, the characteristics of primary process being inferred largely from dreams. Thus the disregard for space and time and for ordinary logic is typical of primary process; the processes that Freud called the dream-work are primary ones, especially condensation, displacement, and symbolization.

6. Primary process involves "mobile cathexis" and the manipulation of large quantities of energy; secondary process involves "bound cathexis" and operates with small amounts of energy. The interaction between primary and secondary processes is conflictual, involving repression, defense, and the like.

7. Primary process is compelling, peremptory; secondary thought ac-

tivity (practical thought, rational thought) we can "take or leave" (Rapaport, 1959, p. 76).

8. Primary process thinking in conscious subjects may be found "either out of strength or out of weakness" (Holt & Havel, 1960, p. 267). That is, primary process thinking may emerge out of ego weakness (as in a psychotic state) or because a person regresses to primary process thinking for fun or in order to open himself to creative ideas. This has come to be called "regression in the service of the ego" (Kris, 1952; Schafer, 1958).

Here then is a rich store of ideas. For these ideas to become a part of general psychology we need, first, to understand the theory in its own terms, second, to criticize it, and eventually, to reconstruct it. The ultimate contribution of Freud does not rest on a decision whether he was right or wrong; eventually we want to know more than he knew, but if he helped to stimulate the search that will tribute enough to him.

FREUDIAN CONCEPTIONS EXAMINED

Let us pass quickly over some of the general ideas that in one form or another everyone finds acceptable. Some kind of *genetic-developmental* theory of thinking is acceptable, and a number of these have of course been proposed, such as those of Piaget (1955) and Werner (1948). The details are a matter of some uncertainty, but there is probably some kind of continuous development rather than a saltatory or discontinuous transition from one stage to the next. The Freudian theory can be conceived in this continuous way, for primary process is never completely displaced by secondary process (Burstein, 1959). Freudian conceptions have been compared with those of

Piaget by Wolff (1960).³ Also some kind of contrast between prelogical, concrete, impulse-driven thinking and more abstract, dispassionate, realistic thinking (both forms found in the adult), is acceptable. It is important here, however, to know just what we are talking about, and Freud rests his case on the dream as the prototype of primary process; this can be objected to either on the grounds that dream thinking is not a good representative of illogical and fallacious thinking (even though it manifests these characteristics), or that Freud gave a one-sided picture of what dream thinking was like. French, who believes that dreams are attempts at problem solving and are more orderly than Freud thought, has reanalyzed Freud's Dora case in these terms (French, 1954, pp. 10-18).

The most controversial features of the Freudian scheme, either because they are unclear, unproven, or disputed, seem to me to be: (a) the theory of the interplay between the pleasure principle and the reality principle, especially in the negative definition of pleasure as tension-reduction, and the separation between affect and cognition, as implied in the notion that affect discharge is an alternative to hallucination as a

³ While the tenor of Wolff's monograph is that the coordination of Piaget and Freud should be rather easy, he has given some penetrating analyses of their differences, particularly in the first stage of development, where the primary-secondary process distinction is most cogent. Here he points out that according to Piaget the organism's fundamental tendency is to assimilate the environment to itself, while Freud's theory is that it tries to rid itself of all stimulation (Wolff, 1960, p. 60). The development of ego-psychology within psychoanalysis now makes it easier for the classical analyst to accept early interaction with the environment, while not giving up any of his long-held views about intrapsychic processes.

means of primary tension reduction; (b) the conjecture that the infant hallucinates the absent incentive; and (c) the energetics involved in the contrast between primary and secondary processes. Each of these deserves some comment.

The tension-reduction theory of motivation has come in for a number of attacks, and attention has gradually shifted from the negatives of tension relief to the positive role of incentives (e.g., Hilgard, 1956, pp. 427-433; White, 1959). Freud's pleasure principle, while somewhat more complex than the typical motivational theory of the experimental students of learning, subjects Freud to the same kind of criticism, for example, for his neglect of joy and hope among the affects (e.g., Schachtel, 1959, pp. 19-21). This issue is being fought out within general psychology, and it would not take too much doctoring to fit the Freudian theory to whatever the outcome is.

A most original feature of Freud's theory is that the infant hallucinates the absent object. This is of course conjecture, based upon the predominantly visual nature of dreams, but the conjecture occurs repeatedly in Freud's writings. It should be noted that this cannot be mental activity at its earliest, for the hallucination is a *memory*, and some theory of prior perception and recovery is implied. The pleasure principle may be conceived to operate before modes of thought have developed at all. At one point Freud used the illustration of the bird inside the egg, with the nutrients there to be had immediately; the wish for nutrients cannot be distinguished from the availability of nutrients (Freud, 1958). Through some further steps, made necessary in human development because the object of gratification is not

always there, *attention* and *memory* develop, and, out of them, *thought*. This is the course of development in the direction of the reality principle, but one thought-activity is split off: that is fantasy making. This is then the primary process that persists in thinking after secondary process thinking has also developed. The fact that the fantasy does not actually bring relief means that secondary process thinking must develop almost simultaneously; we are probably dealing with a ratio of the two processes from the start, more primary process gradually giving way to more secondary process.

How plausible is the conjecture that the infant hallucinates in response to its needs? Evidence would be hard to get, although working backwards by analogy from EEGs and eye movements in hallucinating adults we might be able to get some evidence; to my knowledge this does not exist. The truth is probably a metaphorical one, emphasizing the tendency of thought to move to the concrete, the specific, the pictorial, and attributing to the infant what is found in adult dreams and in the hallucinations of deprived adults (the mirage on the desert) and psychotics. The tendency for more primitive thought to take concrete forms is not without support in experimental studies, for example, the concrete-abstract distinction of Goldstein and Scheerer (1941), and the greater ease of attaining concrete over abstract concepts in general, for example, Heidbreder, Bensley, and Ivy (1948), and Grant (1951). Freud apparently was not completely satisfied with his treatment of hallucinations; at one point he suggested that the *negative* hallucination (i.e., denying the presence of stimulation) might be a better point of departure than the

positive hallucination from which to start an explanation (Freud, 1957).

The energy concepts within Freudian psychology are difficult ones at best and pose a number of problems (Colby, 1955; Hilgard, 1962). The term *cathexis* in Freudian theory is used for some kind of energy charge, but the analogy with physical energy is not a close one; the meaning is much more that of *interest*, or *attention*, or of Lewin's *valence*. In any case a highly cathected idea comes to awareness (i.e., can be attended to) in competition with less cathected ones and can be driven out of awareness by counter cathexes. The notion of *mobile cathexis*, used in discussing primary process, is that, as Holt and Havel (1960) put it, "an idea and its cathexis are easily parted"—the search pattern or drive that can cause one idea to be cathected may as well cathect another one. Hence one idea easily substitutes for another in a dream. An idea and its cathexis are more closely bound when secondary process operates: when one idea is searched for in memory, or somehow comes across the threshold because of the state of its cathexis in relation to competing ideas, it comes in stable, reliable form. Poetry tends to deal in more mobile cathexes than science does ("to take up arms against a sea of troubles" versus "sea water contains sodium chloride"). Dealing with the distinction between primary and secondary process in terms of cathexes is metaphorical, but it communicates something that is comprehensible; still one is never sure but what he is missing something.⁴ In addition to mobile and

bound energy there is neutralized energy (Freud, 1961; Kris, 1950), referred to as delibidinized, deaggressivized, or sublimated. These forms are all said to have their roots in the innate drives (sex and aggression) but have been transformed from primary process so as to be at the service of secondary process; there may also be forms of neutralized energy that do not come from drives (Hartmann, 1950). Once neutralized energy is accepted the dichotomy between primary and secondary processes becomes less sharp (Rapaport, 1959, p. 92).

Another problem of energy in primary and secondary processes has to do with *amounts*, large amounts being involved in primary process, small amounts in secondary process. This is a little confusing because in physical outcome primary process tends to go on while the person is immobilized in sleep and incapable of putting out much energy; secondary process permits the physical outcome of energetic control over the environment. It is necessary to be repeatedly reminded that we are talking about amounts of psychic energy and not physical energy. Actually the matter has not been stated quite properly here: in primary process the quantity of energy dealt with is large because it is mobile and all discharged at once. This is what gives primary process manifestations their insistent quality; they, so to speak, "take over." The total quantity of energy dealt with in secondary processes

is small and when it is also profound. Attempts to clarify the concept have thus far not been very helpful (e.g., Rapaport, 1959, pp. 125-129). There is no doubt that the notion of cathexis attempts to deal with deep psychological problems, e.g., how the registration of a past experience stored in the nervous system becomes available to consciousness, how symbolization occurs. The question is how well it *solves* these problems.

⁴Obscure ideas sometimes seem less obscure to those who use them simply because they become familiar. Cathexis is, in fact, a very obscure idea; as in the case of other obscure ideas it becomes a difficult problem to determine when such an idea is merely ob-

may be the same, but its *regulation* is through small quantities of energy, just as a small thermostat may control a large heating plant. Hence secondary process is more finely tuned and can be turned on and off as primary process (usually) cannot be.

In order to take these ideas out of their metaphorical context and place them nearer to general psychology, we can look for some resemblances to familiar ideas:

1. Free association is more like primary process than controlled association because in controlled association we insist on bound cathexes, that is, on "appropriate" replies, as when we ask for a part-whole relationship, or a large-small relationship, and then give one member of a pair and ask for an associate. In free association, anything will do, so long as an answer is given. Under these circumstances (and this is where Freud comes in) unconscious factors are likely to provide the missing intermediaries between stimulus and response.

2. Some persistent ideas (as in obsessions) have about them a driven quality, as though we are helpless about them; they seem to happen from without, as though they happen *to* us rather than *by* us. Thus we do not feel ourselves to be the stage managers of our dreams. This is what is meant by the immediate and powerful discharge of primary processes.

3. We sometimes distinguish the affective consequences of punishment from the informative consequences. Too much affect may produce what Thorndike called irrelevant emotion; according to the Yerkes-Dodson law too much punishment interferes with learning. Thus the massive involvement of affect is inhibiting to realistic cognitive activity; if the affect comes

in smaller doses, then the organism can profit by it in learning its way around. Here we have a clue to Freud's notion that secondary process experiments with small amounts of energy. The notion is also related to modern information theory, which distinguishes between the control mechanisms and the power operations that are controlled. Rapaport has noted this possible parallel (Rapaport, 1959, p. 91).

4. The opposition between primary and secondary processes is tempered somewhat in the notion of regression in the service of the ego earlier referred to; it is a regression from which one can escape, so that it does not have the full peremptory quality usually assigned to primary process. That is, we can go to a "kid party" and then change our clothes and become adult; we are not committed to hebephrenia by this act of temporary regression. The original discussion of regression in the service of the ego (Kris, 1952) is a very sketchy one; the best elaboration is by Schafer (1958). There is a curious quality about Schafer's account, however. He gives six conditions facilitating regression in the service of the ego; these are all conditions of good mental health or ego strength, and as he reviews them himself he sees that they are not quite appropriate to gifted artists, comics, and scientists (who are supposed to use regression in the service of the ego unusually well). He resolves this problem by indicating that such regressions may serve different individual purposes. The trouble is probably not with his account but with the concept itself. Probably more is involved than that a regression permits primary process thoughts to appear. One might think of several possibilities, such as (a) a capacity for regressive experiences, for ex-

ample, richness of imagination; (b) a tolerance for regressive experiences, for example, lack of anxiety when thought and imagination are given free range; and (c) skill in the utilization of regressive experiences, for example, ability to convert fantasy into acceptable artistic or other creative products, including humor.⁵ These, or other aspects, may mean that the experience called regression in the service of the ego has several dimensions. Schachtel (1959, pp. 244-248) objects to the notion that the experiences are regressive at all; a certain openness to new ideas need not be regressive, but is better interpreted, he believes, as progressive.

When all the trappings of the theory of primary and secondary processes are removed there remains much in the major distinction that is plausible and familiar: enough to invite an examination of the more obscure conceptions.

SOME QUESTIONS SUBJECT TO ANSWER

Let us now grant that as reference-concepts the primary and secondary processes are useful, and see how we can go on from there, outside the special framework of the Freudian metapsychology. The basic classification, following David Rapaport, is between *drive organized* and *concept organized* memories as they enter into our thought processes. If primary process rules out thinking, the vehicles of thought, the ideas to which we can attend, are brought to awareness by the impulses or drives that are stirred up; thus our memories are drive organized. If my reactions to my boss are dictated by an unperceived relation between him

and my father, then my thoughts of the boss are drive organized. If secondary process rules my thinking, then I may use what I have learned from interacting with my father, but I know my boss is not my father, and I react to him in accordance with the demands of the actual social situation. In this case, my thought is concept organized, according to the lines of command within the organization in which I work, the assignment I am working on, and so on. We have long been taught to distinguish between *reasoning* and *rationalization*; the former representing thought under the conceptual mode, the latter thought that is impulse driven.

If we grant the distinction between primary and secondary process, or drive organized and concept organized thought, then we have to decide how we are to use this distinction in talking about the wide range of things people do when they think. There are two chief ways of using a twofold scheme of this kind, one as a *dimension*, the other as a *mixture*.

The dimensional scheme takes off from the notion of growth, and assumes that primary process is primitive and early, secondary process more mature and later. One can then draw a line with primary process at one end and secondary process at the other, and place any act of thought along this line. The thoughts that are represented in the middle are *fusions*, if you wish, with some aspects of primary process and some aspects of secondary. I suppose one could go to a modern art exhibit and place the pictures along such a continuum, with the totally nonrepresentative pictures at one end, corresponding to impulse, with photographic representations of reality at the other; those in between would be the kinds of distorted or stylized pictures that combine impulse with

⁵ Some of these distinctions have been made by As, O'Hara, and Munger (1962) in attempting to discover regression-like experiences related to hypnotic susceptibility.

reality. This scale would be a kind of analog of a scale from primary to secondary process. The dimensional position is the one favored by Rapaport (1951a, 1951b), Hartmann (1950), Kris (1952), Holt and Havel (1960).

The *mixture* scheme suggests that primary process and secondary process remain to some extent distinct, but one intrudes upon the other; their conflicts are compromised in various ways, but there is characteristically enough vacillation between them to keep their identities intact. As one grows older a larger part of his thought tends to be of the secondary process kind, but he reverts to primary process thinking in dreams and fantasy.

These two ways of schematizing the relationship between primary and secondary process can only be distinguished if the conceptual models are clear, for it is often hard to tell the difference between a fusion (implied in the dimensional scheme) and a mixture (implied when the two processes fight it out, but each continues its own existence).

These notions are too abstract to deal with unless we have some examples before us. Let us consider some examples of thinking.

1. A schizophrenic patient says to his physician: "I am 75 years old." The physician says to him: "You feel that you have suffered three times as much as most 25-year-olds." If the interpretation is correct, the patient has distorted reality, assigned himself a false age, as an expression of affect. But in so doing he has multiplied 25×3 correctly. The primary process interpretation is that the ideas that he manipulates come from his store of memories by way of impulse. He does not remember the age based on his birth certificate; he remembers the phenomenal time

through which he has suffered. The fact that he can manipulate these ideas correctly does not deny their primary process origin.

2. A hypnotized subject is told that he is about to hear a very funny joke. The hypnotist tells him: "The whale is the largest living mammal." He laughs as though his sides would split. Aroused from the hypnotic state he is asked why this was so funny. One subject says: "It really wasn't funny. I just had a sort of laughing fit." Another says: "You should have seen the funny whale I pictured with a long snout and tiny legs. It sure was funny!" In the first of these, impulse and cognition were not fused, in the second they were.

3. A hypnotized subject is shown a small metal box with one real light on the left, but told that there are two lights, one on the left and one on the right. He sees both lights. Asked if they are both real, he says, "Yes." Told that one of them is *not* real, but to find which is which, he says: "The one on the right is not real; it casts no reflection in the metal surface, as the one on the left does." If the hallucination signifies primary process, the successful problem solving is secondary process. Here both go on simultaneously, but they remain distinct; the hallucination is not destroyed by the knowledge that it is not real.

4. A subject who volunteers to be hypnotized for the first time by a technique in which gradual eye-closure is suggested, raises his arms before his chest, moans, and sobs. Roused from hypnosis, he can give no account of any ideas associated with the display of affect. In a later interview outside hypnosis childhood memories were reviewed, and he demonstrated how he cowered in a chair when he was beaten by his mother. He re-enacted in the inter-

view the positioning of his hands, his tightly closed eyes, his moaning and sobbing. His behavior in the hypnotic situation can be interpreted as the reactivation of a memory (non-verbal reactivation in this case) on the basis of some similarities between the hypnotic induction and the earlier submission to authority. This memory was drive organized rather than concept organized; it did not, however, involve hallucinations.

5. A subject who has just undergone a hypnotic session without very much success, when leaving the experiment suddenly experiences a spontaneous regression: she finds that her body is shrinking and she is becoming a small-sized girl again. Somewhat frightened by this distorted body-image, she looks about her to see that the world of objects has not changed, and she becomes her own size again. She is able to switch the experience on and off. For a while her regressed body-image coexisted with a real world; it is an important principle that in a regressed state not everything is regressed.

I have here given five illustrations to show what kinds of problems are to be faced in trying to assess primary and secondary process thinking, particularly in formulating them clearly enough to decide whether one should talk about fusions, or mixtures, or both.

Perhaps these illustrations themselves suggest experimental problems. I should like to suggest that more careful study of fantasy productions, eidetic images, and hallucinations will make important contributions, provided these studies are guided by theory. Hypnotic experimentation, from which most of my illustrations were drawn, provides a convenient way of getting into these areas, but other methods are available. Robert Holt and his associ-

ates, for example, have been studying primary process manifestations in Rorschach responses (Goldberger, 1958; Goldberger & Holt, 1958; Holt, 1956; Holt & Havel, 1960). Presumably there should be more secondary process in the TAT, and this might be a good place to examine the problem of fusion versus mixture.

Let me say a word about eidetic images. These have been very little studied in recent years, yet they can be detected when they are looked for. We find a good deal of evidence of their presence among our more hypnotizable subjects. The subject who was told stories by an Irish grandmother who believed in (and had actually seen) Leprechauns, has little trouble in seeing Leprechauns herself, as eidetic images. These are now memory images from childhood, but they bring a kind of gratification that is close to the original meaning of primary process, even though the gratification is derivative from the grandmother. The subjects in our sample who have these images tend *also* to be highly verbal and communicative, by contrast with the nonhypnotizable subjects who lack both fantasy and easy verbal expression. One might suppose words to be representative of secondary process, but they are heavily loaded with primary process too. Thus poetry, a verbal art, uses many of the same devices as the dream. There are many problems here.

A symposium is a good place to throw out problems for discussion, even though answers are hard to come by. Let me summarize some of the issues:

1. Is it possible to sharpen the characterization of primary and secondary process thinking so that the delineation will be clearer than it now is? For example, when is hallucination an essential part of primary process thinking?

2. In dealing with any illustration of thinking that we wish to classify in primary-secondary process terms, do we do better to describe *aspects* of the thinking as primary and secondary functioning, so as to place the illustration on a continuum, or do we describe the mixture and vacillation between the two processes? Or do we need a more complex model that encompasses *both* fusions and mixtures?

3. What kinds of experiments can we set up to help us sharpen these distinctions and bring them into line with our other ways of conceptualizing thinking and problem solving? For example, Charles Fisher's (1960) perceptual experiments suggest the possibility that less clearly perceived (perhaps subliminal) material tends to be recovered in memory through drive organized memories, while more clearly perceived material tends to evoke concept organized memories. Here is certainly the kind of hypothesis that can be put to test, once our criteria of the two types of organization are clearly formulated.

SUMMARY

1. Freudian psychology is in many respects a cognitive psychology, concerned as it is with hallucinations, dreams, memories, symbols, and dis-

tortions of the thought process. It is at once a cognitive psychology and a psychology of motivation.

2. The distinction between primary and secondary processes is a very central one within psychoanalytic theory. The nature of these processes as described by Freud, and interpreted by Rapaport, is best summarized by asserting that there are drive organized and conceptually organized memories that enter into the two kinds of thinking.

3. Some of the problems of the Freudian theory are examined, and the plausibility of the theory is considered in the light of other approaches to the same phenomena. The theory is plausible, but much of its theoretical basis is obscure.

4. Some illustrations are given of the kinds of thought situations that raise questions about the two processes, whether a particular example should be viewed as a fusion of the processes or as a mixture of them. The answer is not clear, and a complete model might have to include both fusions and mixtures, if the distinction between the two processes is to be retained. There are empirical approaches to the problems possible by way of projective tests, hypnosis, and the experimental study of perception and dreams.

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VISUAL DEPTH DISCRIMINATION IN ANIMALS¹

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Visual depth discrimination may be defined as differential response to stimuli at different distances from the observer. Methods for eliciting such responses, and thereby assessing the discriminative capacities of various subhuman species, have been placed in five general classes: The terms "depth" and "distance" are used synonymously throughout, to refer to physical distance.

SIZE CONSTANCY STUDIES

Retinal image size is an unreliable indicator of the depth of objects. For this reason, studies which attempt to demonstrate accurate depth discrimination must show differential responses to cues other than retinal image size. One way of demonstrating the capacity of an animal to react discriminatively to depth cues other than retinal image size is by experimentally demonstrating "size constancy." Size constancy is typically obtained in the following way: first, the animal is trained to approach S^D and not S^A , where S^D and S^A are two stimuli identical except for size, placed at an equal distance from the animal. Then the behavior of the animal is observed when the distances of the two stimuli are changed so as to invert the normal relation between distance and retinal image size. If the animal still approaches S^D , it is clear that this response is under the control not simply of retinal image size, but

of some combination of other cues to depth.

Herter (1930, 1953) trained a single carp (*Carassius vulgaris*) to swim toward the larger of two white circles (17 and 25 mm. in diameter) that were placed at a distance of 25 cm. from the subject. He then tested the animal in extinction under three conditions: (a) the stimuli were placed at 25 and 50 cm., respectively, from the starting position, thus inverting the size-distance relation; (b) the left eye

TABLE 1
NUMBER OF RESPONSES TO THE TWO
STIMULI UNDER THE THREE CONDITIONS

Condition	Stimulus	
	Large	Small
Binocular, unequal distances	9	1
Monocular, unequal distances	4	16
Monocular, equal distances	9	1

of the fish was removed surgically, and the stimuli remained at 25 and 50 cm.; and (c) the monocular fish was tested with both stimuli at 25 cm. from the starting position. The number of responses made to the two stimuli under the three conditions is shown in Table 1. The animal responded correctly (swam toward the larger stimulus) on most of the trials, except when viewing stimuli at unequal distances with monocular regard, where 80% of the responses were incorrect. Herter concluded that the cues for depth discrimination in the carp are binocular, since the animal exhibited size constancy only when allowed binocular vision. Meesters (1940) replicated Herter's

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procedure with the three-spined stickleback and concluded that binocular vision is necessary for depth discrimination in this species as in the carp. Götz (1926) and Hertz (1928) studied depth discrimination in the chick and the bluejay, respectively, by training the birds to approach the larger of two stimuli and then testing for size constancy by placing the stimuli at unequal distances so that the size of the retinal image was no longer an accurate cue to depth. Their animals, which were allowed binocular vision, responded correctly in the size constancy test, showing that chicks and bluejays discriminate depth. However, it is not possible on the basis of these two studies to specify any of the necessary cues for depth discrimination. Köhler (1915) observed the size constancy effect in chimpanzees responding to rectangles of different sizes; however, depth discrimination failed under monocular regard.

INSTINCTIVE MOTOR RESPONSE STUDIES

In addition to the size constancy studies described above, a second way of establishing that an animal has the capacity to discriminate depth is to observe the occurrence and accuracy of instinctive response patterns that require depth discrimination in order to occur. For example, Pumphrey (1948), Grinnell (1921), Hess (1950, 1956, 1961), Benner (1938), and Bird (1926) have interpreted the pecking response in birds as an indicator of depth discrimination, because the occurrence of pecking probably implies that the animal has discriminated the distance to the object pecked. Pumphrey and Grinnell noted that birds that seek stationary food always move their heads back and forth in a type of peering move-

ment, fixating the object to be pecked before the pecking response occurs. This observation led Grinnell to speculate that motion parallax (rate of displacement of the image on the retina) is an important depth cue used by birds in localizing objects, since side-to-side peering necessarily gives rise to differential parallax as a function of the distance of the object.

Benner (1938) placed photographs on the ground in front of some chicks. He found that photographs of peas with the shadows all pointing in the same direction would elicit a pecking response, but photographs in which the shadows pointed in diverse directions would not elicit the response. Hess (1950, 1961) designed an experiment to test whether pecking responses to light and shading depth cues are learned or unlearned in chicks, by raising two groups of chicks under different kinds of illumination, and noting differences in their subsequent pecking behavior. The experimental group was raised in a home cage with a wire mesh floor and glass-bottomed feeding trays, in which the illumination was from below. The control group was raised in a similar cage, except that the ceiling was wire mesh instead of the floor, and the illumination was from above. At age 7 weeks, all chicks were taken from their home cage and placed one at a time in a diffusely lighted test cage. A photograph of some grains of corn was placed on a homogeneous flat surface in front of the chick. The photograph was divided vertically in equal halves, with no visible contour of division. In one half, the shadows cast by the grains on the surface pointed upward, and in the other half they pointed downward. Hess simply observed whether the chick pecked at the photograph and, if so, at which half. As expected, animals in the experi-

mental group (home cage illumination from below) pecked at the grains whose shadows pointed up, and animals in the control group (home cage illumination from above) pecked at the grains whose shadows pointed down. In a second experiment, Hess repeated the above procedure, with two modifications: (a) all animals were tested weekly from ages 1 to 8 weeks, and (b) the illumination in the test cage, no longer diffuse, always came from the opposite direction than it had in the cage in which the chick was reared. In this experiment the proportion of chicks that pecked differentially at the two halves of the photograph increased during the first 6 or 7 weeks. Hess concluded that the control of pecking by light and shade is acquired and that the direction of incidence of illumination determines for the young chick the nature and rate of development of the pecking response to these cues.

In another experiment (Hess, 1956), chicks wore goggles with prismatic lenses arranged so that stimulus object images were displaced towards the subject. When a grain of corn was suspended in the air directly in front of a chick wearing these goggles, its pecks at the grain were observed to fall short in every case. This finding was the same for newly-hatched chicks, chicks 6-8 weeks old, and 2-3 month old chicks who had been restricted to monocular vision since hatching. Hess concluded that chicks employ binocular cues in visual depth discrimination, and that the binocular basis of the discrimination is not learned.

Bird (1926) undertook an evaluation of the role of maturation in the development of the pecking response in chicks. Three groups of chicks at different mean ages were studied: an 8-hour-old group of animals, and two

30-hour groups. Animals in one of the 30-hour groups were "not allowed" any practice in pecking. It was found that chicks in the 8-hour group typically emitted slow, oscillatory pecks, while the 30-hour-old chicks showed rapid, definite pecks. Bird concluded that maturation is an important factor in the development of pecking behavior.

There are several other studies of the pecking response in chicks that were designed to assess the effects of maturation and learning. Most of these studies follow the pattern of the Bird (1926) experiment described above. Their findings confound the effects of sensory and motor development and hence throw little light on the acquisition of discriminative capacity. It is probably true that birds only peck when they can discriminate distance; however, when a young bird fails to peck it may be because it is unable to discriminate distance, but it may also be because its motor capacities are insufficiently developed, while its discriminative capacities are in fact adequate and perhaps amenable to measurement by some other means. In other words, the ability to discriminate depth may be a necessary but not a sufficient condition for the pecking response to occur.

In an attempt to study the very early development of visual depth discrimination, Fishman and Talarico (1961a) subjected prematurely hatched chicks to an approaching and receding optical stimulus (a ball-point pen) during the first 10 seconds of visual experience, and noted the reflex occurrence of blinking responses. When the ball-point pen was brought rapidly to within less than an inch of the chicks' eyes, the number of blinks was not significantly greater than in a control group not exposed to the approaching

stimulus. In a further experiment (Fishman & Tallarico, 1961b), chicks aged 3 hours were tested in a similar fashion, using two types of stimulus: a rapidly approaching comb, and a pleated fan held at a fixed distance and extended in a plane perpendicular to the chick's line of regard, so that the visual angle increased suddenly. In this experiment the response was defined as a reflex movement of the head, away from the stimulus. It was found that the approaching comb produced a greater number of responses than the extended fan or the control condition (no stimulus), and that there were no significant differences in the performance of light- and dark-reared chicks. Fishman and Tallarico regarded these results as tentative evidence for the occurrence of innate visual depth discrimination in chicks at age 3 hours. They also concluded that a change in visual angle alone is insufficient for the discrimination to occur.

Baldus (1926) studied depth discrimination in insects by observing food-getting responses of the larva of the dragonfly (*Aeschna Cyanea*) in order to make inferences about the basis of its depth discrimination. Larvae of this species remain motionless with their front legs folded until prey is within reach (about 10 mm. for an animal 40 mm. long), and then abruptly unfold the legs and seize the prey. If the seizing response is accurate, the animal evidently discriminates the distance to the prey. Baldus tested larvae individually in a miniature aquarium, using stimuli mounted on the end of a wire and placed just under the surface of the water in the larva's field of vision. Flies, very small tadpoles, bits of tinfoil, and other small objects were used as stimuli; emission of the seizing response proved to be independent of

the form of the stimulus. One group of subjects was allowed normal binocular vision; subjects in the other two groups were reduced to monocular vision, in one group by surgically removing a single optic ganglion, and in the other by covering one eye with black paraffin. Insects in the group with binocular vision exhibited accurate seizing responses when the stimulus was moved back and forth in front of them, but insects in the two monocular groups responded, almost regardless of distance, to stimuli subtending the same visual angle as that subtended by normal prey within reach. Baldus concluded that depth discrimination in this species is under the control of binocular stimulation. The paraffin group was used as a control against the possibility that the single-ganglion preparations were showing a motor inhibition resulting from physiological damage rather than an inability to discriminate monocularly the distance to the stimulus. Scheuring (1921) observed the behavior of the codfish (*Gadus virens*), which in feeding on smaller fishes exhibits a propulsive thrust of just the force necessary to carry the animal to its prey. He inferred from the accuracy of the response that codfish discriminate distance rather finely. No attempt was made to isolate cues or otherwise to study the conditions necessary for the attack response to occur.

Plateau (1888) placed insects of several species individually in one end of a darkened box, with two lighted rectangular openings at the other end, and noted the opening through which each insect escaped. One opening was in a fixed position, and the other was mounted near the edge of a slowly revolving disc that formed part of the end-wall of the box. The experiment was designed to test the conjecture that the insects would

be more likely to respond to the moving stimulus, and would escape through the revolving opening. This turned out not to be the case, when the two openings were of equal size and equally illuminated. However, Plateau did find that whenever one of the openings was either larger or more intensely illuminated, it was chosen more often regardless of whether it was the moving or the stationary opening. He speculated that size and brightness may have operated as illusory distance cues, and that the insects were responding to the apparently nearer of the two openings. At any rate it is clear that size and brightness, which normally serve as cues to depth in other species, were discriminated by these insects. Among the insects tested in Plateau's apparatus were the honeybee (*Apis mellifera*), the bumblebee (*Bombus hortorum*), the blue horse fly (*Calliphora vomitoria*), and two varieties of butterfly (*Pieris brassicae* and *Pieris napi*).

JUMPING STAND STUDIES

When an animal leaves a jumping stand, it is possible to study its depth discriminative capacities by observing events associated with the jump under various experimental conditions. The original study of this type was done by Richardson (1909), who trained rats to jump from one platform to another. She measured for each rat the number of trials until jumping was "facile," and gave descriptions of the jumping behavior of the rats. She concluded that visual input is not sufficient for accurate depth discrimination in rats who have not had opportunity for learning.

Russell (1932) devised a jumping stand which measured the horizontal force exerted by an animal leaving the stand. Using this apparatus, he did a thorough study of the conditions

necessary for rats to discriminate distance. First, he established that rats are capable of depth discrimination, by measuring the horizontal force exerted by rats jumping to a platform whose distance was varied between trials over a range of 20 to 45 cm. from the stand. There was a black rectangle on the platform which served as a target. The data showed force and distance to be monotonically related in the range of distances studied. Russell also observed a direct relation between distance and apparent disinclination to jump.

Next, Russell measured the difference limen for distance, for both albino and pigmented rats, by finding the smallest difference in distance between two platforms that always elicited jumps of different forces. The DL as determined by a modified method of constant stimuli was 2 cm. for pigmented rats and 4-5 cm. for albinos. Finally, he eliminated cues to depth one at a time and also in certain combinations, in an attempt to discover the critical stimulus dimensions controlling the discrimination. There was some evidence that binocular cues were important: the rats' eyes seemed to converge before jumping, suggesting that they were utilizing binocular convergence; and it is known that the rat's optic tract decussates only partially and that a binocular field exists. Rats with one eye enucleated, however, were able in this experiment to perform almost as well as binocular rats, and the force-distance relation was again observed in the monocular condition. This suggests that binocular cues are not necessary. The effect of size cues in depth discrimination was tested in a series of trials in which the size of the black rectangular target and the width of the platform were systematically varied; compensatory changes were also made in the dis-

tance between the platform and the jumping stand, so that the retinal image of the target was the same size on all trials. Although the retinal image remained the same, the force exerted by the rat in leaving the jumping stand increased as the distance to the target increased. This finding shows that rats discriminate distance in the absence of size cues.

Motion parallax may also have been an important cue in depth discrimination in the above experiments, since almost all the rats exhibited head-swinging or peering behavior before jumping. To test the crucial character of this cue, a homogeneous white ground was placed behind the platform to preclude any contours which might furnish differential parallax relative to the platform. Nevertheless, the rats were able to discriminate well, as measured by the force-distance relation. However, since animals in this series had been used in previous trials, practice effects were confounded with experimental treatments, so that it could not be concluded unequivocally that the removal of motion parallax cues does not affect discrimination. The only other cue to be experimentally manipulated was texture or aerial perspective, which was degraded by lowering the level of illumination for a series of trials. Under this condition, the discriminative behavior of the rats deteriorated noticeably. However, lowering the illumination may degrade other cues to depth besides aerial perspective; for example, light and shade. Thus, the decrease in discrimination may not have been due to the change in aerial perspective alone.

When Russell (1932) eliminated depth cues in combination, he found that average absolute force in jumping was positively correlated with

the number of cues removed. That is, when several cues were removed, the ordinal distance-force relation was preserved, but the overall average force was greater, so that the rats tended to overjump. This result suggests that discriminative ability is to some extent a function of the number of cues present. In his concluding series, Russell placed the apparatus in the dark, with the target which had formerly been black now a lighted rectangular opening serving as the only source of illumination for the animal. Thus, all cues except retinal size were held constant as distance was varied. In this situation the rats were unable to discriminate distance (jumping force no longer correlated with target distance).

This series of experiments led to the conclusion that rats can discriminate distance in the absence of any single cue but that discrimination is more difficult when several cues are removed in combination. Lashley and Russell (1934) later used the same apparatus in an experiment to determine whether rats' ability to discriminate distances is "innately organized." Each rat was raised in darkness to age 100 days; on Day 101 it was brought into the light and given a single trial in which the jumping stand and the platform were separated by 5 cm., followed by five trials at 20 cm. The next day it was given five trials at 20 cm., three at 40 cm., three more at 20 cm., and finally three more at 40 cm. The following day it was given nine trials: one trial each at distances of 24, 26 . . . 40 cm. The results showed an overall ordinal correlation between force and distance, and also an average DL which was not significantly different from that of Russell's animals; Lashley and Russell (1934) concluded that "the visual perception of distance

and gradation of force in jumping to compensate for distance are not acquired by learning" In a further experiment using the same procedure, Lashley (1937) found that lesions in the optic thalamus or colliculi did not significantly impair depth discrimination in rats.

Greenhut and Young (1953) repeated the experiment of Russell (1932), and found that the force with which rats left the jumping stand did not correlate very highly with the distance to the target platform, when the distance was varied randomly between trials. They obtained a substantial force-distance correlation only when the distance to the target platform was varied systematically on successive trials; i.e., gradually increased or gradually decreased. On the basis of this finding, these authors suggested that serial order effects may have accounted in part for Russell's results. Greenhut and Young further noted that the horizontal force exerted by rats leaving the jumping stand was not always an accurate indicator of the actual distance traversed by the animal. They proposed a weighted combination of horizontal and vertical forces as a more valid dependent variable for future studies employing the jumping stand method. In a subsequent study, Greenhut (1954) showed that rats discriminate stimulus objects at different distances in a two-choice alley runway.

Wallace (1959) performed a series of experiments to study depth discrimination in the desert locust (*Schistocerca gregaria* forskål). While in its natural habitat this locust very frequently swings its head and upper thorax back and forth from one side to the other, in what appears to be a visual scanning or orienting response. The purpose of Wallace's study was

to determine whether this scanning behavior is responsible for depth discrimination. He used a miniature jumping stand and two platforms each with a black rectangular target. Pretests showed that the insect will always jump to the nearer of the two platforms. In the first experiment the sizes of the rectangles were varied along with their distance so that both rectangles had the same proximal image size. In the second experiment the nearer rectangle always cast a smaller image. Under both of these conditions the locusts almost always jumped to the nearer platform. Wallace concluded that the necessary cues for depth discrimination must come either from binocular vision or from the differential parallax resulting from the locust's scanning responses (a monocular cue). To examine the first possibility Wallace replicated his experiments using insects rendered blind in one eye. These insects also jumped to the nearer platform, showing that binocular vision is not a necessary condition for depth discrimination in this species. Wallace concluded by exclusion that the basis for the discrimination is the scanning response which gives rise to differential motion parallax. Further support for the critical role of the scanning response was furnished by a fourth experiment employing a single target which moved slowly from side to side. It was found that insects jumping at a moment when the target was moving in a direction opposite to the scanning movement tended to jump short, whereas insects jumping at a moment when the head and target were moving in the same direction tended to overjump. This extremely interesting result is exactly what would be expected if the locust's distance discrimination were based on the rate at

which an image travels across the compound eye.

Forel (1910) has suggested that sharpness of object contour may operate as a cue to distance for insects, whose lenses are rigid and have a fixed focus, since the amount of blur of an outline on the insect's retinae must have a direct relation to the distance of the object. This possibility was not mentioned by Wallace, and might account for the results in his first three experiments. However, it does not account for his findings with moving targets.

PHYSICAL CLIFF STUDIES

Depth discriminations may be observed in animals by recording certain properties of their behavior when confronted with a cliff or precipice. However, visual, somesthetic, and kinesthetic sensory inputs may be confounded in studies employing this method.

Yerkes (1904) placed tortoises individually on a 30X60 cm. board at various heights (30, 90, and 180 cm.) above a net, and measured the interval between the time the animal was placed on the board and the time it fell off the board and into the net. He observed three species of tortoise on this apparatus; a water tortoise (*Chrysemys picta* Schneider), an amphibious tortoise (*Nanemys guttata* Schneider), and a land tortoise (*Testudo Carolina* Linnaeus). Four tortoises from each species were each given one trial a day for 10 days. The average time per trial (in minutes) spent on the board by animals of the three species at the three heights is shown in Table 2. The total number of animals among the three species that stayed on the board for 60 minutes at the three heights is shown in Table 3. Both of these sets of data show clear main effects for height

and for species. Yerkes (1904) interpreted the height effect as evidence that vision plays a part in determining the responses, and the species effect as a reflection of differences in the evolutionary importance of depth discrimination for animals of the same family who live in different

TABLE 2
AVERAGE MINUTES PER TRIAL SPENT ON
THE BOARD BY THREE SPECIES OF
TORTOISES AT THREE HEIGHTS

Height	Species		
	Water	Amphibious	Land
30 cm.	0.6	27.6	42.7
90 cm.	6.3	49.1	54.2
180 cm.	10.1	60.0	59.2

TABLE 3
NUMBER OF TORTOISES OF THREE SPECIES
WHO SPENT 60 MINUTES ON THE
BOARD AT THREE HEIGHTS

Height	Species		
	Water	Amphibious	Land
30 cm.	0	11	9
90 cm.	0	30	33
180 cm.	1	40	39

environments. Evolutionary differences may have been confounded, however, with differences in the amount of experience land and water tortoises had had in situations resembling Yerkes' physical cliff. Thorndike (1899) made similar observations on chicks aged 95 hours. He placed them individually on platforms at varying heights above the table, and noted when they jumped to the table. He reported that chicks always jump quickly from heights between 1 and 10 inches, hesitate a long time before jumping from a

height of 22 inches, and almost never jump from a height of 39 inches. He concluded that chicks discriminate depth innately; however, it is not clear to what extent he controlled the visual experience of his subjects. In a similar experiment, Kurke (1955) gave domestic chicks 10 days of "enforced vertical experience" in a special brooder containing a raised platform. When tested on another platform whose height varied, these chicks jumped down to the table from a greater average height than chicks given only "restricted vertical experience." Waugh (1910) placed mice individually on a small disc attached perpendicularly to a movable vertical rod passing through a hole in the table. He then raised the disc to various heights above the table and observed how much time elapsed before the mouse jumped down. A mild electric shock was used to induce the animal to jump. Waugh found a positive correlation between the time to jump and the distance to the table, and inferred that mice discriminate depths up to 18 cm., which was the greatest height he used. It is apparent, however, that he left uncontrolled the kinesthetic and vestibular sensory inputs afforded the animal during the ascent of the disc.

VISUAL CLIFF STUDIES

A principal defect in the cliff studies cited above is the confounding of visual with nonvisual stimuli. Recently, studies of a similar nature have been done using a simple apparatus which eliminates all nonvisual cues, thus enabling the experimenter to obtain discriminations based solely on visual stimuli. The apparatus, known as the visual cliff (Gibson & Walk, 1960), consists of a runway board with a large sheet of clear glass directly underneath it,

extending out on either side, parallel to the floor. The runway and glass are in a large box several feet square, and are attached to the sides of the box at about 2 feet above the bottom. The bottom of the box is covered with some pattern, for example a red and white checkered pattern. Paper bearing the same pattern covers the underside of the glass sheet on one side of the runway, which is called the shallow side since the optic pattern is almost level with the runway. The other side is called the deep side, since the patterned surface is about 2 feet below the runway. Animals are tested for depth discrimination by placing them on the runway and noting to which side they go if they leave the runway. Latencies and number of times the runway is crossed may also be measured. When animals consistently choose one side over the other, they are certainly responding differentially to whatever visual depth cues the pattern affords. Clearly, two classes of experimental variables may be manipulated in studies using this apparatus; one is the optic relation between the two fields adjoining the runway, and the other is the past history of visual stimulation of the animals who are tested.

Gibson and Walk (1960, 1961) and Walk and Gibson (1961) tested chicks, turtles, rats, lambs, goats, pigs, kittens, and dogs, using a visual cliff with a red and white checkered pattern, and found that all these species reliably chose the shallow side of the visual cliff. Chicks and goats always went to the shallow side at age 24 hours, and cats did the same at age 4 weeks. Cats placed by hand on the deep side showed freezing behavior, which did not extinguish even after many trials. Tallarico (1961) observed a tendency for chicks to

choose the shallow side as early as 3 hours after hatching.

In a series of control experiments, Gibson and Walk (1960) covered the underside of the glass in the visual cliff with plain gray paper on both sides of the runway. In this situation, the proportion of rats leaving the runway on either side was not significantly different from one-half. As an additional control, tests were made with no paper under the glass on either side, so that both sides were deep. Rats in this series simply failed to leave the runway.

Two major cues were presumed to determine side preference: relative motion parallax (rate of displacement on the retina of the images of the pattern elements on each side as the animal moves along the runway) and the size of the retinal images of the pattern elements. Gibson and Walk next endeavored to isolate these cues and thus to assess their importance. Differential image size was eliminated by making the pattern elements on the shallow side smaller, so that the pattern elements on the two sides gave rise to retinal images which were equal in size. With motion parallax presumably the only remaining differential depth cue, day-old chicks and adult rats almost invariably chose the shallow side. It will be noticed that other cues to depth remained besides the supposedly isolated motion parallax (e.g., differential accommodation). Next, differential motion parallax was eliminated by covering the underside of the glass on both sides of the runway with patterned paper containing elements somewhat smaller on one side than on the other, thus isolating size as a depth cue. In a series of trials under this condition, day-old chicks chose the two sides about equally often; adult rats tended to choose the side with larger pattern elements.

Walk, Gibson, and Tighe (1957) tested 90-day-old light- and dark-reared rats on the visual cliff, and found that animals from both groups reliably chose the shallow side, supporting the interpretation that depth discrimination in rats can occur without prior learning. Nealey and Edwards (1960) noted that the dark-reared animals in the Walk et al. (1957) experiment had been allowed a 20-minute period to adapt to the light before being tested. These authors performed an experiment designed to control for the possibility that very rapid learning, analogous to imprinting, had occurred during that time. They replicated the Walk et al. procedure, obtaining the same results. Two additional control groups were tested; one was a 90-day dark-reared group, for whom the 20 minutes of light adaptation took place under individual translucent hoods, thus excluding any opportunity for pattern-vision. Seventy-five percent of the rats in this group chose the shallow side of the visual cliff; this finding supports the Walk et al. conclusion that depth discrimination occurs independently of learning. In the second control group, the rats were surgically blinded, to control for any nonvisual cues that might have been present. These rats did not discriminate between the shallow and deep sides.

Nealey³ also replicated the Walk et al. (1957) study, but with dark- and light-reared rats 10 months old, and a pattern consisting of only three wide lines on either side parallel to the runway. These lines all subtended the same visual angle when viewed from the runway. He found that the light-reared rats chose the shallow side, and the dark-reared rats

³ S. M. Nealey, personal communication, 1961.

TABLE 4
SUMMARY OF CLASSES AND METHODS USED IN EXPERIMENTS ON SUBHUMAN VISUAL
DEPTH DISCRIMINATION

Method	Class				
	Insects	Fish	Reptiles	Birds	Mammals
Size constancy	Wallace	Herter Meesters		Götz Hertz	Köhler
Instinctive response	Plateau Baldus	Scheuring		Benner Bird Fishman & Tallarico Grinnell Hess Pumphrey	
Jumping stand	Wallace				Greenhut & Young Lashley Lashley & Russell Richardson Russell
Physical cliff			Yerkes	Kurke Thorndike	Waugh
Visual cliff			Gibson & Walk Walk & Gibson	Gibson & Walk Walk & Gibson	Gibson & Walk Nealey Nealey & Edwards Tallarico Walk & Gibson Walk, Gibson, & Tighe

failed to make the discrimination. The same dark-reared rats were tested immediately thereafter, using the original Walk et al. stimulus pattern, and still did not discriminate. This suggests that the rats raised in the dark for 10 months may have suffered impaired vision. The impairment was apparently temporary, however, because when the rats were tested after a month in the light, they consistently chose the shallow side. In a subsequent experiment, Nealey³ tested the hypothesis that the Walk et al. rats discriminated on the basis of sharpness of focus. He projected

sharp and blurred patterns of equal size onto the two sides of a ground glass screen which was in the position originally occupied by the clear glass sheet in the visual cliff. It was found that light-reared rats descended equally often on both sides, thus ruling out focus as the critical cue controlling rats' behavior on the visual cliff.

CONCLUSION

Table 4 summarizes the research reviewed in this paper, showing what classes of animals were studied by the different methods. The capacity to

react discriminatively to the distance of a visual stimulus appears to characterize a great many species, ranging from insects to primates. Especially in the case of insects, birds, and rats, it is evident that displacement of the images on the retinal mosaic is a very important factor in depth discrimination. Wallace (1959) demonstrated with the desert locust the importance of visual scanning behavior (which necessarily gives rise to motion parallax), and Baldus (1926) found that the dragonfly larva responds to mov-

ing stimuli, but never to stationary stimuli. Pumphrey (1948) and Grinnell (1921) noted the occurrence of scanning responses in birds in the natural environment, and Gibson and Walk (1960) showed the critical importance of motion parallax in depth discrimination by chicks, performing under more controlled conditions. These findings support the general statement that stimulus image displacement and the resulting motion parallax are intimately linked with depth discrimination.

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VERBAL MEDIATION AS A FUNCTION OF AGE LEVEL

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On the basis of her study of transposition in the two-stimulus problem in young children, Kuenne (1946) proposed that

there are at least two developmental stages so far as the relation of verbal responses to overt choice behavior is concerned. In the first, the child is able to make differential verbal responses to appropriate aspects of the situation, but this verbalization does not control or influence his overt choice behavior. Later, such verbalizations gain control and dominate choice behavior (p. 488).

Similarly, Kendler, Kendler, and Wells (1960) suggested in a study of reversal and nonreversal shifts in discrimination learning in preschool subjects (Ss) that

there is a stage in human development in which verbal responses, though available, do not readily mediate between external stimuli and overt responses (p. 87)

and noted that Luria (1957) had reached essentially the same conclusion. This suggestion that there is a stage of development in which verbal responses do not serve as mediators is designated the "mediational deficiency hypothesis" in the present paper. The evidence regarding this hypothesis is reviewed in the following sections.

Reversal and Nonreversal

Kendler and Kendler (1959) reasoned that if verbal mediation occurs, performance on a reversal shift (in which the previously positive stimulus becomes negative; and the previously negative stimulus, positive) should be superior to that on a nonreversal shift (in which the previously relevant dimension becomes irrelevant; and the previously ir-

relevant dimension, relevant). In the reversal shift, appropriate verbal labels developed during the original discrimination remain relevant and facilitate learning; whereas in the nonreversal shift, new labels must be acquired and the old labels, developed in the original discrimination, interfere with learning. The verbal labels function like the "internal orienting responses" of Goodwin and Lawrence (1955), which involve the "identification of and reaction to" the relevant dimensions.

Kendler and Kendler (1959) reported that kindergarten Ss who learned the initial discrimination rapidly, compared with those who learned it more slowly, were superior on the reversal condition and slightly inferior on the nonreversal shift. They concluded that the Ss were in the process of developing mediating responses relevant to the task, and that some Ss were further along than others, since the fast initial learners responded as though they were mediating, and the slow initial learners responded as though mediation did not occur. Kendler, Kendler, and Wells (1960) found that preschool Ss performed like the slow-learning kindergarten Ss, and that instructions to verbalize their choices on the last 10 trials of the initial discrimination had no effect on reversal or nonreversal shifts, leading to their statement of the mediational deficiency hypothesis.

O'Connor and Hermelin (1959) found that imbeciles learned a reversal faster than normal preschool Ss except when the imbeciles were required to verbalize their choices on

the initial discrimination. Verbalization interfered with reversal by imbeciles. The imbeciles in the verbalization group and the normal preschool Ss performed in essentially the same way as those of Kendler et al. (1960). O'Connor and Hermelin interpreted their results as indicating that the use of verbal labels interferes with reversal shifts, because the S must inhibit not only the association between the overt choice response and the previously positive stimulus, but also the association between the choice response and the *name* of the previously positive stimulus. This interpretation does not conflict with that of Kendler and Kendler (1959) if it is assumed that in normal Ss the mediator is an orienting response, usually verbally directed, which involves identification of and reaction to the appropriate dimension, and that in imbeciles it is a verbal response functionally equivalent to a nonsense-syllable name (as in studies of acquired distinctiveness of cues). Kendler and Kendler assumed that S names both stimuli as members of a single dimension, and O'Connor and Hermelin assumed that the imbecile names only one stimulus. It should be emphasized that the performance of O'Connor and Hermelin's normal preschool Ss supports the mediational deficiency hypothesis, since it is in line with the findings of Kendler et al. (1959, 1960).

Discrimination Set

In the preceding section it was assumed that the mediator in reversal shifts is a verbal orienting response. Two studies of discrimination set (orienting responses, identification of relevant dimensions, etc.) provide somewhat conflicting evidence regarding the assumption.

Weiss (1954) found that set-inducing instructions (informing Ss that the reward was always behind the same stimulus in a discrimination task) were more effective in older than in younger preschool Ss. This finding supports the assumption, since it supports the mediational deficiency hypothesis. On the other hand, Spiker (1959) found that although pretraining with distinctive stimuli facilitated learning a discrimination with similar stimuli, there was no significant age difference in the effectiveness of the pretraining. If the pretraining resulted in the acquisition of a discrimination set, the data contradict the mediational deficiency hypothesis or the assumption that discrimination set (orienting response) involves verbal mediators. However, as Spiker noted, the pretraining may serve only to minimize failure-produced responses by minimizing failures. The failure-produced responses are incompatible with efficient discrimination performance and their occurrence results in inferior performance. His results, then, do not necessarily contradict the mediational deficiency hypothesis.

Transposition

Kuene (1946) hypothesized that possession of a concept of the relation between the stimuli in a discrimination task would facilitate transposition because the concept would mediate correct responses. Using the two-stimulus problem, she found that the frequency of transposition on a "far" test, several steps removed from the training stimuli on the stimulus continuum, increased with increasing age level, but her data suggested that possession of the concept by younger preschool Ss did not facilitate transposition as much as it did in older preschool Ss. Alberts

and Ehrenfreund (1951) obtained essentially the same results in a similar study. Both of these studies, as well as others (Jackson, Stonex, Lane, & Dominguez, 1938; Terrell, 1958; Terrell & Kennedy, 1957), found a high frequency of transposition on the "near" tests (one step removed) in the two-stimulus problem, with no significant age differences. According to Spence's (1936) theory, mediation is not required for transposition on the near test; and Jackson et al. (1938) noted that

although the subjects [3-6 years of age] may readily understand the concept "bigger than," they do not transfer by such verbal analysis on critical trials [on a near test] (pp. 581-582).

Hunter's (1952) study, designed to test absolute versus relative theories of transposition, apparently demonstrated transposition in preschool Ss who did not have the relevant concept. Hunter attempted to design the tasks in such a way that absolute theories would predict no transposition, but the Ss may have acquired a discrimination learning set (violating the boundary conditions of absolute theories of transposition) since he trained them on three discrimination tasks before giving them the transposition test. Shepard (1957) found that discrimination learning set was maximal in preschool Ss after they were trained to criterion on one problem. Hunter's results might be attributed to a discrimination learning set, but they might also be accounted for by Stevenson and Bitterman's (1955) hypothesis that *S* transposes if he fails to discriminate between the training and test sets of stimuli. The latter possible explanation is less plausible than the former, since Hunter had marked differences between the training and test stimuli in some conditions.

Plenderleith (1956) found no significant differences between normal and feeble-minded Ss (mean mental age about 69 months) in the acquisition of a discrimination learning set or in the subsequent acquisition of a discrimination reversal set (except when there was a long interval between sessions). Although other studies (Ellis, 1958; Kaufman & Peterson, 1958; Koch & Meyer, 1959; Stevenson & Swartz, 1958) have found a relationship between mental age and speed of acquisition of discrimination learning set, there is no evidence that verbal ability is directly involved. Therefore, if the transposition in Hunter's (1952) Ss resulted from discrimination learning set rather than the usually postulated mechanisms, his finding that preverbal Ss transposed would not be relevant to the present topic. In support of this conclusion, Levinson and Reese (1961) found no significant age difference in the speed of acquisition of discrimination learning set, indicating that it is at least less dependent on age level than verbal mediation is.

Three studies of transposition in the intermediate-size problem tested the distance effect (i.e., the decrease in frequency of transposition with increasing separation between the training and test stimuli on the stimulus continuum) in young children. Reese (in press) and Rudel (1960) found no significant difference between preschool Ss who had the concept of middlesizedness, or at least the concept name, and those who did not. A significant distance effect was obtained in both studies. Similarly, Reese (1961) found that on a far test (three steps removed from the training stimuli) younger (preschool) Ss transposed only when the area ratio of the stimuli was small, i.e., when the transposition did not theoretic-

cally require mediation (Stevenson & Bitterman, 1955), though older (kindergarten) Ss transposed on the far test even when the area ratio of the stimuli was large, when mediation was presumably required. The results of the last study support the mediational deficiency hypothesis and the Stevenson-Bitterman hypothesis. The obtaining of the distance effect in both "concept" and "no-concept" groups in the first two studies can be explained by the Stevenson-Bitterman hypothesis if it is assumed that mediation did not occur in the concept Ss.

Spiker, Gerjuoy, and Shepard (1956) interpreted their results as indicating a greater frequency of transposition in concept than in no-concept Ss, but their procedure was such that mediation need not be assumed to have occurred, since acquired distinctiveness of cues could account for their findings. That is, in their study learning could have been facilitated by acquired distinctiveness in the concept group, even if mediation did not occur, but there would be no acquired distinctiveness in the no-concept group.

Acquired Distinctiveness

Studies of acquired distinctiveness of cues (stimulus pretraining) in young children have uniformly found no significant deficiency in the effectiveness of the pretraining for the younger Ss. Norcross and Spiker (1957) found that younger preschool Ss made fewer correct responses than older ones, but stimulus pretraining was equally effective in both groups, i.e., age did not interact significantly with experimental conditions. Spiker (1956) found that stimulus pretraining was more effective in younger than older preschool Ss. The younger control group was inferior to the

other groups, but the other groups performed at about the same high level. Weir and Stevenson (1959) obtained a similar result with their preschool Ss, but the interaction between age and experimental conditions was apparently not significant. Finally, Cantor (1955) found no significant effects of age levels in his study of stimulus pretraining in preschool Ss.

The interpretation of the results of these studies must take possible ceiling effects into account. The maximum mean percentages correct responses in the studies were: Norcross and Spiker, 83%; Spiker, 86%; Weir and Stevenson, about 90%; and Cantor, 79%. It appears that there were ceiling effects, particularly in Spiker and Weir and Stevenson. If there were ceiling effects, the effectiveness of stimulus pretraining for the older experimental groups would have been obscured. It is also possible that the older control Ss used pre-experimentally acquired names for the stimuli, obscuring the stimulus pretraining effect in the older experimental groups. In either case, no definite conclusion could be made about the relative effectiveness of the pretraining in younger and older preschool Ss.

Acquired distinctiveness does not involve mediation, according to Dollard and Miller's (1950) interpretation, and therefore a failure to find a deficiency in younger preschool Ss would not contradict the mediational deficiency hypothesis. However, Spiker (1956) has suggested that acquired distinctiveness may result from the use of the stimulus names for rehearsal of the stimulus-response connections during the inter-stimulus interval. His interpretation requires mediation, since the rehearsal would not facilitate performance un-

less the stimulus name mediated the appropriate response when the stimulus was presented. According to his interpretation, then, a failure to find a deficiency in younger Ss would contradict the mediational deficiency hypothesis. Since the results of the previous studies are inconclusive because of the possibility of ceiling effects and the possible occurrence of pre-experimentally acquired stimulus names, it is apparent that further study of the acquired distinctiveness of cues is required.

There is no clear-cut evidence regarding the mechanisms which have been assumed to underlie acquired distinctiveness. Jeffrey (1958a) found that learning names for two stick figures, one pointing to the right and the other to the left, was facilitated in preschool Ss by learning to push buttons toward which the figures pointed. The response unit was present during the transfer task, and Jeffrey reported that one S lifted the appropriate shoulder before naming, but other Ss "would look at the appropriate button before supplying the name of the figure" (p. 274). Whether mediation or acquired distinctiveness was involved is not clear. Jeffrey (1958b) has also obtained facilitation of learning to associate buttons with piano tones by pretraining Ss to match the tones with a piano or by singing. In this study, however, the facilitation may have resulted from the development of a discrimination set. In studies in which the response unit was not present during the transfer task, pretraining with motor responses has not produced facilitation. For example, Murdock (1958) found that motor pretraining did not produce facilitation in college students; and Reese, in an unpublished study, found no facilitation of fifth graders' learning to

associate buttons with colored stimuli following pretraining in which the stimuli were associated with switch throwing responses. Although the Ss of the former and latter pairs of studies differ in age, the presence or absence of the response unit during the transfer task may be the critical variable determining whether or not facilitation occurs on the transfer task.

Acquired Equivalence

There have been only two studies of the acquired equivalence of cues in preschool Ss, and both of these also involved acquired distinctiveness. In both studies it was impossible to separate the effects of acquired equivalence and distinctiveness experimentally or statistically. Jeffrey (1953) reported that pretraining with verbal and motor responses led to greater facilitation of performance in older than in younger preschool Ss (the groups were divided on the basis of MA, which apparently also yielded a division on the basis of CA); and Shepard's (1954) data implied a similar result, since she obtained a correlation of .70 between errors on a transfer task and trials to criterion on the name learning task. Although she did not report the correlation between age level and trials to criterion, age level should be negatively correlated with trials to criterion and therefore negatively correlated with errors on the transfer task. Since studies of acquired distinctiveness, which does not necessarily involve mediation, have found no deficiency in younger Ss (see above), the Jeffrey and Shepard studies may be interpreted as indicating a deficiency in younger Ss in acquired equivalence, which does require mediation, supporting the mediational deficiency hypothesis.

Double Alternation

Although it is usually considered that a series of double alternations requires mediation, Hunter and Bartlett (1948) found that of 11 Ss below the age of 48 months, 9 failed to reach criterion on a double alternation (the 2 who reached criterion were 43 and 45 months of age), but no S younger than 60 months gave the basis of responding either spontaneously or in response to questions asked at the end of training. Stolurow and Pascal (1950), studying double alternation in mental defectives, also reported that the solution of the problem did not always indicate ability to verbalize the correct pattern of response. The implications of these results are inconclusive, however, since Bugelski and Scharlock (1952) have shown that even college students may mediate without being able to verbalize the process, i.e., without awareness. If mediation occurred without awareness in the younger Ss who reached criterion in the Hunter and Bartlett (1948) and Stolurow and Pascal (1950) studies, and if the other younger Ss also possessed the concepts required ("left" and "right"), the results may be interpreted as supporting the mediational deficiency hypothesis.

CONCLUSION

Studies of reversal and nonreversal learning, transposition in the two-stimulus and intermediate-size prob-

lems, acquired equivalence of cues, and possibly other problems indicate that there is a deficiency in mediation in young children, compared with older children. The studies reviewed above indicate that the critical age for the occurrence of mediation may be different for different experimental situations and for different concepts. It seems likely that in some cases the deficiency is a characteristic of an early stage of human development, but that in others it may be a characteristic of an early stage of concept formation. There is some evidence that inadequately learned stimulus names, if used for rehearsal (as suggested by Spiker, 1961), produce interference. Reese's (1960) data suggest such a trend in fourth, fifth, and sixth grade school children, and McCormack's (1958) study suggests it in college students. It is proposed, then, that with a well-learned concept there is no necessary deficiency in mediation as a function of age, but with a less well-established concept there is a deficiency at any age. (For a discussion of the possible sources of deficiency with inadequately learned concepts, see Spiker, 1961.)

If mediation is a "voluntary" process, as rehearsal is, there may be a stage of development in which Ss have typically not yet learned to use it, and instruction in the use of the process should facilitate the learning of these Ss. If it is an involuntary or automatic process, instructions should have no effect.

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PROPORTION OF LIGHT TO CYCLE AS A DETERMINANT OF CRITICAL FLICKER- FUSION FREQUENCY

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One of the main variables which affect the critical flicker-fusion frequency, CFF, is the proportion of light to the total cycle, P_L . This determinant is usually indicated by the term "light:dark ratio," LDR. Although it is completely specified by either of the two expressions LDR or P_L , the latter will be used here, for reasons given later. Landis (1954) concluded from a survey of work on the problem that the relationship between CFF and P_L was confused and stated that "no-one has offered the beginning of a solution" (p. 274). He found that inconsistent results made theoretical interpretation impossible.

The present paper will indicate that consistent results are now emerging and will evaluate two approaches to their representation and interpretation. The first approach is the conventional one which plots CFF scores in cycles per second. The second approach, not previously described, uses different functions of the same data to give simpler relationships between variables. These relationships can in turn be related to other visual tasks and make possible further tests of hypotheses on the nature of the mechanisms of visual fusion.

TERMINOLOGY

CFF is defined as the frequency at which an intermittent light appears fused on 50% of trials. Terminology and relationships between CFF variables can be set out in two sections, one applying at all constant frequen-

cies and the second to the particular frequency of CFF, the latter being specific cases of the former. The time values are expressed in seconds or milliseconds.

1. At all constant frequencies:
 P_L = proportion of light to cycle,
 P_D = proportion of dark to cycle,

$$P_L + P_D = 1$$

LDR = light:dark ratio,

$$\text{LDR} = \frac{P_L}{P_D} = \frac{P_L}{1 - P_L}$$

t'_C = time of one cycle, t'_L = time of one light flash, t'_D = time of one dark period,

$$t'_C = t'_L + t'_D$$

2. At critical flicker-fusion frequency: t_C = critical cycle time, t_L = time of one light flash, t_D = time of one dark period,

$$t_C = t_L + t_D$$

If CFF and P_L are known, then the values of t_L and t_D may be found by the following formulae:

$$t_L = P_L \cdot \frac{1}{\text{CFF}}$$

$$t_D = P_D \cdot \frac{1}{\text{CFF}}$$

$$= (1 - P_L) \cdot \frac{1}{\text{CFF}}$$

Further relationships between these variables can be derived but the most important have been given above.

Different expressions have been used by various authors for the concepts of t_C and P_L as defined above. For example, Brown and Forsyth (1959) used the term "period" instead of t_C while Bartley has used several terms for the concept. These include: critical flash interval (1937), critical flicker interval (1951), and cycle length and intermittency cycle (1958). The term "critical cycle time" is used here because it conveys the exact meaning required with emphasis on time at fusion.

The expressions used to denote P_L include: light:dark ratio (Landis, 1954; Ross, 1938; Winchell & Simonson, 1951; etc.), flash duration or pulse-to-cycle fraction (Bartley, 1937, 1958), relative duration of light interval or light flash duration (Crozier & Wolf, 1941), and light-time proportion (McFarland, Warren, & Karis, 1958). The most common of these terms, light:dark ratio, is often erroneously applied to P_L values. Thus, Lloyd and Landis (1960) defined light:dark ratio as duration of light interval/total cycle time (p. 334), which corresponds to the definition of P_L given above. The values they gave as LDR's, viz., .005, .01, .25, .50, .75, and .95 are P_L values, not ratios of light to dark periods. Similarly, Landis (1954) applied the term, light:dark ratio, to values ranging from .1 to .9 and Battersby and Jaffe (1953) to the figures 20%, 50%, and 80%, which are obviously P_L values. This incorrect use of the term, light:dark ratio, is confusing; particularly since it was correctly defined by the authors in other sections of their reports.

Bartley and Nelson (1960b) supported an earlier use of the term, pulse-to-cycle fraction, instead of light:dark ratio (Bartley, 1958), with the objection that " 'light' and 'dark' "

are experiential response terms, not stimulus terms" (p. 241). This does not seem to be a valid or sufficient reason for rejecting the term, light:dark ratio, whereas the confusion arising when LDR is used also to mean P_L is decisive. It is simpler to express and easier to comprehend immediately P_L values such as .3, .5, and .7 than the equivalent LDR values of 3/7, 1, and 7/3. The term P_L is also preferable to any of the previous terms in that it can be incorporated in a consistent terminology as is shown above. The use of P_L and P_D to refer to proportions, and of t_L and t_D to refer to times at fusion, avoids the ambiguity which arises when t_L and t_D are used for both proportions and times as was done by Crozier and Wolf (1941) and Rutschmann (1955).

BRIGHTNESS COMPENSATION

When P_L is altered systematically, the factor of brightness compensation must be considered. Talbot's Law (or the Talbot-Plateau Law) states that the apparent brightness at fusion, i , of a test patch, is determined by the product of the maximum luminance, I , and P_L ; that is,

$$i = I \cdot P_L$$

This equation has been shown to hold only at and above fusion frequency. If I is changed as P_L is changed, so that the fused brightness, i , as defined by Talbot's Law, remains constant, then this is referred to as a compensated experiment (Cobb, 1934; Lloyd & Landis, 1960; Ross, 1938; Winchell & Simonson, 1951). On the other hand, if I is kept constant so that fused brightness, i , changes with P_L , then this is referred to as an uncompensated experiment (Bartley, 1937; Battersby & Jaffe,

1953; Crozier & Wolf, 1941; Ross, 1943).

The advantage of compensation is claimed to be (Bartley, 1941) that it allows "the effect of the temporal course of the stimulus pattern alone on c.f.f. to be determined" (p. 121) since brightness changes at fusion are controlled to give a constant average amount of light. However, altering I to keep i constant in accordance with Talbot's Law causes differences in apparent brightness *below* fusion. Crozier and Wolf (1941) used these differences below fusion to argue against applying Talbot's Law in CFF studies. They believed the dark period to be important and claimed that compensation obscures the effects of light and dark intervals as well as giving rise to unsymmetrical CFF-Log I curves. They asserted that it is maximum luminance and not fused brightness which is crucial.

Since, by definition, at CFF the subject reports flicker and fusion equally frequently, there would appear to be no a priori reason for selecting either (a) constant apparent brightness above fusion with variable luminance, or (b) variable apparent brightness above fusion with constant luminance. The choice must surely be made on empirical evidence as to which conditions produce the simplest, most meaningful curves that can be related to other visual phenomena.

FIRST APPROACH

Shape of Curves

Recent experiments on the CFF- P_L relationship (Bartley & Nelson, 1961; Lloyd & Landis, 1960) confirm the consistent patterns emerging in spite of differences in stimulus conditions. The contradictions found by Landis in studies prior to 1954 have, by systematic research, been attributed mainly to the factors of com-

pensation, differences in luminance and areas, or to a combination of these. It is now clear that at low luminance levels (uncompensated), the curve of CFF against P_L is bowed,

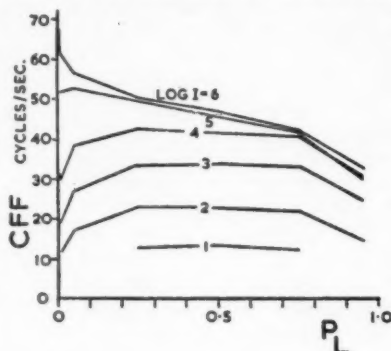


FIG. 1. CFF versus P_L for selected uncompensated luminances—in trolands. (Data for subject D. D., Lloyd & Landis, 1960.)

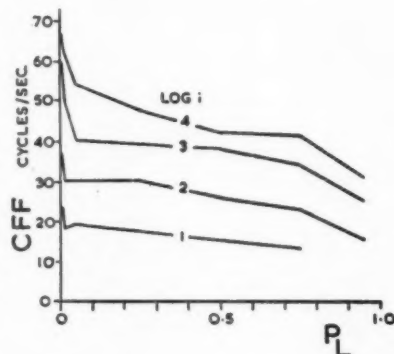


FIG. 2. CFF versus P_L for selected compensated luminances—in trolands. (Data as for Figure 1.)

with maximum CFF occurring at a P_L of .5, while at high luminance levels, the maximum is at a low P_L . Compensation tends to straighten the curves since it requires higher luminances at low P_L 's and therefore raises the CFF scores at these values; compensation causes progressively

less difference in CFF scores as P_L is increased. An increase in area raises all curves on the ordinate.

The general shape of CFF- P_L graphs at different luminance levels is illustrated in Figure 1 for uncompensated, and Figure 2 for compensated luminances. The values used were estimated from the data given in a report by Lloyd and Landis (1960) and are for conditions of 1° test patch area and log luminances as labeled for only one subject, D. D., but his results are considered typical. (The authors give all luminance levels for $P_L = .5$ as .03 log units lower than those for all other P_L values. This unexplained difference is allowed for in the figures but it causes the values for $P_L = .5$ to be approximate.)

Ross (1938, 1943) compared the compensated and uncompensated methods. His results were used by Landis (1954) who, unfortunately, reversed the labels on the graphs and his subsequent discussion is therefore misleading. If the graphs are correctly labeled, it can be seen that as in Figure 2, compensation straightens the curves of CFF against P_L .

Bartley's Model

The majority of studies on the relationship between P_L and CFF reported in the last few years have been by Bartley and his co-workers (Bartley, 1958; Bartley & Nelson, 1960b, 1960c, 1961). Bartley (1958) proposed "a conceptual model of CFF" which was based on an earlier finding (Bartley, 1937) that when CFF is plotted against luminance, the curves for various P_L values cross. Bartley emphasized that at the intersection of two P_L curves, this particular combination of frequency and luminance has two P_L values which are equivalent in producing fusion. He also pointed out that when CFF is plotted against P_L for different luminance

levels (as in Figure 1), the graphs are curved so that for a particular frequency and constant luminance, again two or more P_L 's on the abscissa produce fusion. Bartley's predictions about the shape of these CFF- P_L curves will be discussed in this paper but none of his neurophysiological theorizing will be included.

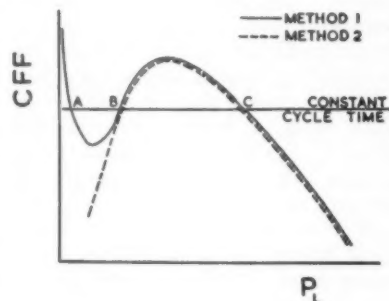


FIG. 3. Schematic curves of CFF versus P_L as predicted for two methods by Bartley's model.

Bartley's earlier model (1958) states that for a constant frequency (or cycle time) at an intermediate luminance, a very low P_L always produces flicker. As P_L is increased, with the cycle time remaining constant, his model predicts that the subjects' reports change to fusion, then to flicker, and finally to fusion. This is shown schematically in Figure 3 in which A, B, and C indicate the three transition points expected. The curve itself represents the CFF values where, by definition, flicker and fusion are reported on half the trials so that fusion is reported at points above the curve and flicker at points below the curve on more than half the trials.

Bartley (1958) stated that each transition A, B, and C "represents what we ordinarily . . . call critical flicker frequency" (p. 112). How-

ever, in the introduction to a later report, he and Nelson (1960b) stated that only transitions A and C (Figure 3) "represent the conditions we usually label as CFF," but transition B, from fusion to flicker, "has no name since no-one else, to our knowledge, has either entertained such a transition or obtained it. We might call it FFF, fusion-to-flicker frequency" (pp. 241-242). It seems unnecessary to give a new name to this fusion-to-flicker transition, since by definition all points on the curve are CFF values and there is no evidence that different results are obtained when P_L is changed at constant frequency than when frequency is changed at constant P_L .

Within a range of "not-too-short" cycle lengths and at a medium luminance level, Bartley's model postulates two or three transition points depending on the method of limits used. Two methods are discussed or implied:

1. To hold cycle time constant and vary P_L .

2. To hold P_L constant and vary cycle time (i.e., frequency)

Each of these two procedures can be performed in two different directions: (a) from flicker to fusion, or (b) from fusion to flicker. The model makes no prediction of the number of transition points when the constant method is used.

Bartley (1958) described Method 1 and claimed that it should yield three transitions, but in the later article, he and Nelson (1960b) stated "by the present method of going from flicker to fusion, only two crossings under any single combination of intensity and cycle lengths are to be expected" (p. 244). The implication is that if Method 2a is used, the CFF- P_L graph will appear as dotted in Figure 3. The hypothesis that different curves are obtained by different

psychophysical methods was not made explicit then by the authors, but they did use what they termed the "direct" and "inferential" methods (i.e., Methods 1 and 2, respectively) in their latest report (1961). The evidence from this and other studies discussed below, indicates that not only do the two methods yield similar results but that the model's expectation of flicker at very short P_L 's is not confirmed in any study.

Lloyd and Landis (1960), for example, using Method 2 and the lowest P_L values to date, namely .005 and .01, did find very slight reversals in the direction of the curve at these values, but closer examination indicates that the inversions apparently occur randomly and are almost certainly experimental error. There are 13 out of 26 possible inversions and these occur at high as well as medium luminances. But of these 13 inversions, 12 occur for the 1° and only one for the 2° area, and 10 of the inversions are reported by one subject. This lack of generality must preclude the results from supporting the model which requires consistent inversions under specified conditions.

Bartley and Nelson's article (1960b), publishing results for seven subjects, gave no details of the observers apart from noting that two were the authors. Possibly, all the other subjects were untrained or naive, since their curves are reminiscent of those given by Nelson, Bartley, and de Hardt (1959) in their article on variability, although standard deviations are not reported. However, the curves of their last subject, BAR, presumably Bartley and therefore a trained subject, are the most regular and the most like other authors' results. By the same token, they also do not conform to expectations of Bartley's model.

In their second report for the two trained subjects from the previous report, Bartley and Nelson (1960c) draw graphs of CFF plotted against P_L and on each graph there are one or more horizontal lines (such as shown in Figure 3) at apparently arbitrarily determined frequencies. Numbers are given both to the point where the lines did cut the curves ("jogs") and also where they did not but were expected to by the model. At the two intermediate luminances where the model should hold, there is no indication of an inversion at the lowest P_L but a number is given in each case to its predicted crossing point.

Bartley and Nelson (1961) suggested that one reason for the absence of expected inversions in their results might have been that the P_L values were not low enough, but even Lloyd and Landis' values of .005 and .01 did not produce consistent inversions. The other explanation which the authors believe is "far more plausible" (p. 44) appears to be that the model as formerly stated is not applicable at low and medium luminances but "it is only at very high intensities that short pulses produce flicker" (p. 45). This change of hypothesis was based on the occurrence of small inversions in the curves of CFF against P_L for four out of five subjects at the highest luminance level but only with the inferential method. The authors do not point out that there is no evidence of any inversions using the so-called direct test of the model, Method 1.

Bartley and Nelson make the unfounded assumption that CFF curves must be irregular and state that no one has previously incorporated these "irregularities" or inversions into a model (1960c, p. 6). They erroneously claim that there were inversions in both their own data and those of Ross (Bartley & Nelson, 1960b, p.

244; 1960c, pp. 6-7). However, the above review of experiments shows that the inversions or irregularities have not been consistently demonstrated under the required conditions in any study.

A SECOND APPROACH

The relationship between CFF and P_L can be examined by a different method not previously investigated, using not critical frequency in cycles per second but the constituent values of critical cycle time, viz., light time and dark time at fusion as defined in the section on terminology. Just as CFF is the critical number of cycles per second at fusion, so t_D is the critical duration of the dark time at fusion. An alternative definition of CFF to the one given earlier is the frequency at which the dark time between light pulses can be discriminated (i.e., flicker reported) on half the trials. Since the cycle time (reciprocal of CFF) is the sum of two variables, t_L and t_D , and on the assumption that the discrimination of t_D is important, these time values can be used in the graphical representation of results instead of the more usual CFF scores. The significance of t_D as a variable is that replotting CFF data in terms of t_D gives graphs which not only appear simpler than those plotted with CFF in cycles per second but are also related to dark threshold curves discussed below.

This approach can be shown in the results of any studies of the CFF- P_L relationship but those of Lloyd and Landis (1960) will be used again. Figure 1 shows results of this study plotted for CFF against P_L (uncompensated) and Figure 4 the same results with $\log t_D$ plotted against t_L . It can be seen that this second method gives almost linear relationships. Further graphs can be plotted in this manner, for example, $\log t_D$

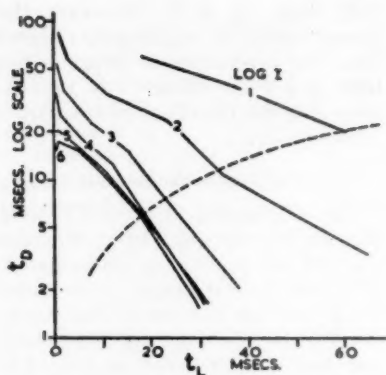


FIG. 4. $\log t_D$ versus t_L for selected uncompensated luminances—in trolands. (Data as for Figure 1; dotted curve gives values for $P_L=0.75$.)

at fusion against area, luminance, or P_L and in each case a regular and simple relationship is found. (The logarithms of t_D values give the best approximation to linearity as may be expected by comparison with other visual functions.) It should be noted that with this method of plotting results, all values with the same P_L will fall on a smooth curve. The curve for $P_L=.75$ has been shown in Figure 4.

It is clear from Figure 4 that increasing time of light and/or luminance decreases t_D . The same can be shown for increasing area of the test patch (unpublished results of the author). It seems that an increase in the total excitation level, whether through time, luminance, or area increases the processes, probably inhibitory, responsible for perception of flicker.

This observation is supported by studies on the "dark threshold" defined here as the dark interval necessary for the fusion of two or more successive flashes, as distinct from "dark time" which refers to the dark interval at CFF. A summary of dark

threshold studies (Bulanova & Luizov, 1954; Dunlap, 1915; Granit & Hammond, 1931; Lichtenstein & Boucher, 1960; Mahneke, 1958) is included here since they illustrate a simpler fusion situation which gives similar results to those from CFF studies and emphasizes the importance of dark time to both situations.

As early as 1915 Dunlap attempted to estimate the shortest perceptible time interval between two flashes of light, and although a second aim was to ascertain the relationship between dark interval and CFF this was not carried out. Dunlap used an episiotister for the tests and was both subject and experimenter. The brightness of the light source was not given but must have been fairly low since the highest CFF was for $P_L=.5$. Dunlap's main result was the decrease of the dark threshold with increase in the length of the first flash. A comparable result was obtained by Bulanova and Luizov (1954) who determined dark thresholds for six observers. They varied exposure time and luminances of the light source and surround and one of their main findings was a consistent decrease in dark threshold with increase in the luminance of the light source.

Mahneke (1958) has improved and enlarged both these studies. He estimated the dark thresholds for 17 different numbers of successive light flashes (2-100) and 6 different flash durations (1, 2, 5, 10, 20, 50 msec.). He again found that dark threshold decreases as light time increases. Mahneke pointed out that although dark time must be an important determinant of CFF, most studies vary times of light and dark together, whereas only Dunlap and he up to that time had varied these independently of each other. He showed that an increase in the number of

flashes decreases the dark threshold and that an increase in the duration of each flash in a series causes a decrease in the dark interval.

Figure 5 shows Mahneke's results replotted as $\log t_D$ against t_L for varying times of exposure. He reported that "the chief reduction in the dark interval . . . takes place during the first approx. 500 msec." (p. 17) and quotes Granit and Hammond's study (1931) in support of this. However, Figure 5 shows more clearly than Mahneke's graph that t_D is still decreasing with exposure times up to at least 2 seconds under the conditions of his experiment, but any longer exposure times would probably not improve the ability to discriminate much further. That the curves for 1- and 2-second exposures are almost identical and approaching linearity is important confirmation of the relationship between CFF and dark interval, since exposure times from 1 to 2 seconds are most common in CFF experiments which use the discontinuous method of limits (see Simonson & Brozek, 1952) or the method of constant stimuli (author's unpublished results).

Lichtenstein and Boucher (1960) pointed out the importance of "critical duration" in various visual phenomena, such as occurs in the summation of subthreshold stimuli, the Bunsen-Roscoe Law, etc. In each of these cases a limiting (critical) value exists for the temporal effects of light energy. The authors studied the critical duration, not of "on" stimuli but of the dark interval between stimuli or the "minimum detectable dark interval." The difference between this and previous studies such as Mahneke's was that the light flashes instead of being physically continuous were them-

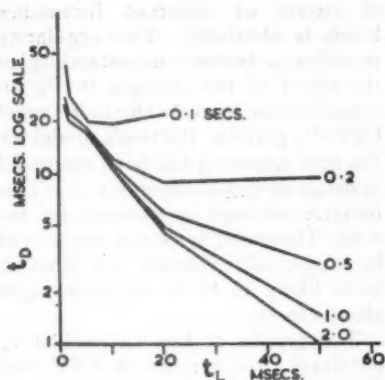


FIG. 5. $\log t_D$ versus t_L for various exposure times at constant luminance. (Data from Mahneke, 1958.)

selves made up of flashes of light which, however, appeared fused. The aim was to compare the effect on the dark threshold of discontinuous but apparently fused light energy with that of light which was both physically and apparently fused as used in previous studies. The same decrease in dark interval with increase in light duration was found.

DISCUSSION

In his review, Landis (1954) indicated that the relationship between CFF and P_L had not, to that time, been successfully incorporated in a theory of CFF and suggested that this determinant may be a "pseudo-problem" (p. 274). Results of studies reviewed by him were inconsistent and there did not appear to be any explanation for the various curves obtained. Consistent results are now emerging and one of the aims of this paper is to show that this pseudo-problem can be resolved by a different method of graphing results. When the data from P_L studies are redrawn, using t_D and t_L as the axes instead of CFF and P_L , a regular set

of curves at different luminance levels is obtained. This regularity provides a better understanding of the effect of the changes in P_L on visual fusion than do the more usual CFF- P_L graphs. Bartley's model for the first approach has been discussed in detail as this is the only recent systematic attempt to account for results. However, it is not supported by empirical evidence nor does it seem likely to be in its subsequent altered form.

The graphs of $\log t_D$ against t_L obtained from studies of CFF and P_L clearly show that increase in time of light, luminance, and/or area of the test patch, results in a decrease in the time of dark necessary at fusion. Similar results have been found in dark threshold studies and the conclusion to be drawn is that any increase in the total level of stimulation reduces the dark time. This seems to involve an inhibitory process and this conclusion is supported by the work of Granit (1947, 1955). His findings on the electroretinogram of man show that increasing the level of energy in each flash, whether by area, luminance, or time, "uncovers" the off-effect which Granit associates with inhibition. The off-effect has been shown to be related to better discrimination of flashes, so that shorter dark times or high CFF are the result.

The possibilities of neurophysiological mechanisms are only briefly stated here to indicate a basis of the t_D - t_L relationship; the main concern of the paper is with perceptual data and their presentation. However, there is observational evidence to support the brief summary given above. Lichtenstein and Boucher (1960) used the fusion to flicker method of limits and as the episcotister disc speed slowed down to

the first report of flicker, the dark interval appeared as a "dark pulse" which was most striking when the "on" stimuli were long. Mahneke (1958) mentioned a similar observation: with a series of flashes each of 50-millisecond duration, he found that a slight alteration in dark interval was enough to change the appearance from clearly fused to obviously flickering, while the same effect was not obtained with the same number of flashes but each of only one-millisecond duration. As Mahneke (1958) commented "this finding also indicates that the capacity of the human eye to discriminate successive light flashes increases with increasing quantity of light" (p. 16).

The second approach to the representation of CFF- P_L results leads to alternative conclusions in some studies. For example, McFarland et al. (1958), in a study of age, CFF and P_L , found that there is a larger difference between age groups' CFF scores at low than at high P_L 's and suggested that sensitivity of CFF to the aging process is enhanced at low P_L 's. Graphing their results using t_L and t_D values, however, shows that increase in age produces a constant increase in time of dark necessary at fusion for any given length of light.

Further experimental work should produce reliable CFF scores at extreme P_L values which are needed, for example, to give conclusive evidence on Bartley's model. However, if an episcotister rather than electronic apparatus is used there are difficulties in obtaining square light pulses at these extreme values (Lloyd & Landis 1960, p. 332), and it has been shown that if square pulses are not produced the CFF scores are affected (Bartley & Nelson, 1960a).

The present paper aims to show the usefulness of presenting CFF data in

terms of time relationships at fusion. Investigations of the effect of changes in P_L on CFF are important because these time functions are varied by

altering P_L and the graphs of light versus dark times lead to a better understanding of the phenomenon of CFF.

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REPORT ON PAPER BY EDWARD GIRDEN ON PSYCHOKINESIS

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I will first state my bias. Going up in the elevator at the Men's Faculty Club at Columbia University years ago, I listened to Harold Urey, Nobel Prize winner, describing to his colleagues how he viewed the problem of replication.

You get an interesting result that you can't explain. You improve the conditions, put in little additional steps, so that your result will be clearer. You can't get the result that you got before. Then you say, "I'll go back and do it exactly the way I did it the first time." So you do that, and you don't get anything like what you got before.

Everyone in science—even a humble science like psychology and a still humbler one, parapsychology—has encountered what Urey was describing. What is the course for the scientist under such conditions? Is it more constructive to say that, because it could not be repeated, the effect was never there? Or is it to say that it was an artifact? Or to say that it was just a challenge, something out on the edge of the known, waiting to be replicated? It seems to me that Girden feels that if a result cannot be replicated, it has to be written down as a fluke. My bias is to question this as the scientist's approach to PK or to anything else.

Psychokinesis is "the direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation."¹ The commonest way to test for its presence is to ascertain whether dice, coins, etc., tumbled against barriers, come to rest with

those faces uppermost which the subject wishes to come uppermost; or, in the case of "placement PK," wishes that the objects (dice, etc.) come to rest on one designated area and not on another. These are two of the three main PK effects reported in the literature. A third effect frequently reported is that successes ("hits") are significantly more frequent in first runs, or in the first parts of a task, than later (the "decline effect") where mean chance expectation would yield a horizontal straight line with no significant slope. It is Girden's task to review the evidence for these three effects.

To begin with something concrete: McConnell, Snowden, and Powell (1955) (Department of Biophysics, University of Pittsburgh) undertook to replicate a series of Duke University studies showing (a) excess of hits where wishing for the various die-faces, (b) decline effects. They used a motor driven cage which whirled dice against a system of internal baffles, and as the cage came to rest at the end of each 180° turn, automatically photographed them. At various times subjects determined in advance for which die-face to "wish." The hits were *not* significantly high, but the decline effects *were* significant ($p = .002$). Granting that much of the early Duke University work of J. B. Rhine (1943, 1944, 1945) and others used loose methods, with hand thrown and cup thrown dice, warranting only a preliminary hypothesis, the first serious problem would seem to be this replication by McConnell et al. The answer, so far,

¹ Definition in glossary of each issue of the *Journal of Parapsychology*.

is partially positive. A second question relates to *further* replication, using this same equipment. Dale and Woodruff (1947), using the same equipment and also other equipment, did *not* get significant results either in total hits or in decline curves. Girden's feeling seems to be that since, when using the same equipment, two investigators did not get a successful replication, the McConnell data leave no positive residuum of evidence for the effect. The p value of .002 for the decline effect reported by McConnell and collaborators to confirm Rhine's various studies, would ordinarily be regarded as safely significant, and even if a correction is introduced owing to the fact that this is a "selected" result, not replicated by Dale and Woodruff, some would regard the hypothesis as tentatively "confirmed." But let us begin by admitting different approaches to procedures of this sort.

The same logic would appear to apply to an earlier replication study by Dale (1946). This study, with equipment described below, entailed the assignment of subjects to groups who tried, respectively, for sixes, fives, fours, threes, twos, ones in a predetermined order so that dice biases which would favor higher scores during part of the work would be balanced by scores disfavored by such bias, and cutting across both individual subjects (all subjects tried for all faces in turn) and permitting the analysis of decline curves for each subject. The excess of hits was significant at the .005 level. The type of decline curves previously noted (consistent declining from beginning to end of a task) did not appear, although a similar phenomenon appeared: most of the excess of hits was in the first portion of the subject's work ("beginner's luck"). The dice were cast down

a chute with many baffles which tossed them about, so that on the first run, as on the last, their position after coming to rest was (normally) wholly unpredictable. Dale added one other check for internal consistency. She did an odd-even correlation by subjects, showing that the same subjects who scored high on odd numbered units of the work tended to score high on the even numbered units of the work, and so on, just as if we were dealing with tests of perceptual or motor skill. It is difficult to see how this measure of internal consistency could be due to biased dice, if one recalls that the irregularities or wear and tear in the dice are a function not of individuality in the psychological sense, but of physical attributes which cut across such individuality.

I have said more than Girden says about the McConnell et al. studies and the Dale studies. Indeed, in many cases his accounts of experimental methods are so brief that it is hard to comprehend what the experimenter was trying to do, what his methods were, and what results he obtained. In the study by Binski (1957), dealing not with dice, but with coins and a roulette wheel, one gets from Girden the impression that the subject's 10 throws of 100 coins each, sometimes trying for one face of the coin and sometimes for the other, gave a "run of luck," that is, within chance expectation. Thus if heads were predicted, there might well be several consecutive throws which gave more heads than tails. Actually, 100 coins were thrown *each time* and there was one individual subject whose trials yielded the desired face in 548 out of 1,000 observations, which is a very significant number of hits; and he did as well or better in each of the next four throws. Statistical treatment must deal *not* as

Girden does with 10 trials, but with a trend involving 10×100 trials and large deviation or "antichance" value. Girden's report also fails to note that the same subject who is unique in these coin results was, by a wide margin, also unique in an experiment involving a standard roulette wheel, so that no gross averaging of either the roulette results or the coin results pooling all subjects would be permissible. In other words, there are in Binski's work two very significant studies of a certain individual, and this has somehow not appeared at all in the condensed statement which Girden offers.

A comparable case is an experiment by Mangan (1954), which is put to one side by noting that it began with a "solo" series, in which the subject is his own experimenter. The point of such a solo series, however, is to get preliminary data, which are then to be extensively checked by having an independent observer and recorder in due time properly recorded. Mangan did this, and the high results were maintained. It is to be doubted whether Girden grasped the purpose of the experiment. Another interesting point that bears on the question of the way in which Girden worked: Mangan, noting the bias of ordinary dice, prepared to do two things; first, to make alternate throws for *high* and *low* dice (e.g., 6's versus 1's), taking an everyday working attitude, and dealing with totals in which the tendency of the low dice to go below chance could be balanced by the tendency of the high dice to go above chance, and obtaining a safe margin of significance for totals by playing one die bias against the other. Mangan then noted the possibility that the *human observer*, as well as the dice, has biases, and offered some tentative data on the summation of such ef-

fects. Repeatedly in this report, indeed, the experimenter's willingness to work with the normal situation of dice bias, to stress it, and to make use of it for experimental purposes, leads only to Girden's comment that the dice were biased.

Now as Girden indicates, there has been controversy about much of the PK research since it was inaugurated in the '30s and '40s, and first saw publication in 1943. Vigorous attacks on "optional stopping," inadequate rotation of die faces, permission to subjects to call any die face they liked, and several other loose and casual procedures, have been rather prominent in the parapsychology literature, and many of the defects have not been vigorously or consistently removed. Girden has done us a genuine service in calling attention to these unrectified errors.

At the same time I would gladly agree that there is no published experiment on PK which involves all the features of an "ideal" experiment as follows: (a) large enough mass of material to be treated by conventional statistical methods; (b) completely impersonal, preferably mechanical or electronic, devices for tumbling the dice, coins, etc., within a compartment from which they cannot escape but in which they can be photographed; (c) instructions to the subject to try now for a particular die face, now for another die face, the instructions being mandatory and the order of targets being determined independently of the subject's choices or habits; (d) offering photographs of the equipment with the subject in place and photographs of the dice when at rest, to be made available to all serious investigators who are interested; (e) the two major hypotheses, first anticipating an overall excess of hits over mean chance expec-

tation, and second, decline curves in terms of functional units of work done by the subject, should be analyzed in the customary manner; (f) the matter of optional stopping applied both to a terminal point for each individual subject and a terminal point for the selection of the last subject who is to be run. It would be tedious to set up a box score to indicate the degree of compliance of each experiment with these specifications. It may be noted that very few psychological experiments, if any, are set up in this way. The more customary procedure is for the experimenter after reading criticisms of his work, to accede as far as possible to such requests, and this I believe the reader will find has been done in the case of the Dale and McConnell experiments and the Forwald-Pratt experiments noted below. When we are dealing with "placement" effects—that is, an attempt to move the dice into a particular position rather than to make a certain face come up the most—the precautions will involve variations, but in general the principles just stated would appear to hold, and I am glad that Girden has stressed them.

It is puzzling to find Girden taking the position with increasing strength toward the end of his paper that no psychological hypothesis is being tested and no psychological conditions investigated for their effects. Hypotheses regarding motivation, satiation for the task, anxiety and self-consciousness are invoked in many of these studies, sometimes stated before the event and then tested, and sometimes offered as afterthoughts, tentatively suggested as material for further work. In fact, in view of the frequency with which one hears the statement that there are no orderly systematic hypotheses in

parapsychology, one might be bewildered to encounter the large number of tested and testable hypotheses offered.

After experimental data have been analyzed, the attempt to cast some light upon what may have been happening is ordinarily considered reasonable, although of course a fresh test of any emerging hypothesis is needed. It seems a little unfair to refer to this standard psychological procedure as "post hoc rationalization" (Girden, 1962). Thus, in view of the large amount of interest in attitude toward a task as influencing success in that task, the suggestion offered by an experimenter that intellectual conflict *might* (*sic*) unconsciously act as a score depressing factor seems a modest one.

In the matter of replication one notices, as one does in work that is half-experimental, half-clinical, the effort to get a psychologically meaningful and highly motivated task, and the willingness to abandon a procedure which seems to be leading into a *cul de sac*. The question then arises whether, along with the shift to a new method, there is a consistent attempt to create psychological conditions which will favor a phenomenon which is so little understood. It seems to me, without this freedom to alter attitudes experimentally, one would not get far in any frontier area. One question, of course, is whether the new hypotheses are pursued long enough. This does not result solely from an oversight. Rather, it results from the intrinsic difficulty of developing workable hypotheses which arise from our present limited knowledge, and keeping experimenters with their noses to the grindstones to pursue these relentlessly, even at the cost of abandoning all the interesting possibilities which occur to experi-

menters in a new field. It is a very human situation. I think one pays a considerable price for the lack of interest in replication; in fact, I have been consistently a pleader for more and more narrowness, and the denial to oneself of challenging new opportunities, because that is the only way in which I could maintain my demand for replication. But he is a pretty wise man who can decide what, in a new science, is the soundest procedure. To accuse investigators, however, of "free wheeling," or lack of hypotheses in papers which abound in suggestions for new work, as are those by Forwald (1952), seems unnecessarily arbitrary.

It is also difficult to understand why there is repeated emphasis by Girden on the point that the cardinal control method must be the comparison of scores made during wishing with scores made under nonwishing conditions. There are many other types of controls. In fact, the comparison of individual die faces *for which one wishes* with other die faces *for which one does not wish* seems to be accepted and stressed by him elsewhere in his paper as a salient and necessary control. Moreover, in the placement tests the area into which one tries to push the dice is compared with the area into which one does *not* wish them to move, and this is properly accepted as a control. Moreover, the questions about decline effects are regarded by him as legitimate questions to raise, although in these cases there is no explicit *wish for a decline* and no wish during the course of the experiment to progress from hits in one position to hits in another position.

The matter of bias in dice is a very old question. It was faced by Rhine and the Duke group at the very beginning of their work. With most

dice the amount of material excavated for placement of the dots results in bias in favor of sixes and for the most part down through with the decline in the value of the dice, so that the Number 1 is the least frequently thrown. Knowledge of dice bias is not, however, always combined with awareness of the fact that differential wear and tear, chipping, smoothing, and other frictional and gravitational factors are fantastically complicated and that such "perfect dice" as are advocated by Scarne (1956) produce their own problems quite quickly. At the American Society for Psychical Research (ASPR), for example, we carefully considered the question, tried out the perfect dice and decided on grounds similar to those of Rhine, that it is very much better to work with an internal control in the experimental design, now trying for one face and now for another, targets being randomized or systematically varied in a manner uncorrelated with wear and tear. Though there were in the early days of the Duke laboratories some unfortunate lapses, the fact remains that unawareness of this phenomenon was not something one could throw at the experimenters, suggesting essentially that "any intelligent school-boy" would know better than to proceed with such dice as are commercially available. Dice bias has been a primary problem all along. There are dozens of situations in psychology in which one uses imperfectly balanced material and "rotates out" the defects by one sort of control method or another. This is what has been done in PK research. Girden has made a valuable point in showing that some of the decline effects may have been due to using too many sixes in early series and too many ones in later series, etc., but this is a specialized

critique of a few specific experiments and does not cover all decline effects, several of which have appeared very clearly even under the conditions of rotated targets just described.

In discussion of the "probability model" Girden feels that experimenters make a "basic assumption" that known factors are randomly distributed and hence, when obtaining deviations from the theoretical predictions, must be dealing with the unknowns. Rather, it would seem to me that investigators recurrently encountering unexplained deviations from a probability model have a clear scientific *obligation* to pursue such matters and try out hypotheses by experimental intervention, that is, intervention through the use of independent variables. "Wishing" is precisely such an independent variable, introduced to see whether the effect is in the expected direction. The same is true of a few cases in which the expectation is that the objects will move in a manner diametrically opposed to that of the wish, and here likewise the hypothesis has been tested.

Now a few more words about replication. In view of the large-scale effort made by Dale (1946) in her "First ASPR Experiment," and the positive results in total hits it is puzzling to learn that the failure of similar results to appear in *later* tests in which psychological and other conditions are somewhat altered, is taken as evidence against the reality of the *first* results. Compare Urey above. Perhaps Dale and other experimenters should go on to the bitter end, repeating and repeating. From the account given it seems likely that this sequence of events characterized her effort: initial enthusiasm, satiation, anxiety, and inability to get back the psychological conditions of the initial success. Is it appropriate psycho-

logically to drive on and on into this hypersatiated atmosphere? There are probably individual differences among psychologists as to how to proceed in such an instance. She repeated the experiment once with some changes in conditions and got no replication of results. From here on the problem is partly psychological—what to do to recapture the working condition of Experiment I—and partly statistical, since one set of results is significant at the .005 level, and the other not significant.

A more dramatic example of Girden's position is his reporting of the extensive experimental work of the Swedish engineer, Forwald (1952). The basic belief that no solo work can ever be sound reappears. The later successful repetition of the subject's work in the presence of witnesses is not enough to get rid of the initial contamination.

Forwald's work, apparently because some of it was solo work, is reported very briefly. He has gradually developed increasing acumen in the development of physical instrumentation for the task and psychological insight as to working conditions which are meaningful and replicable. An illustration is his handling of the discovery of the fact that the table used in Sweden for the placement tests, with a line or wire separating the table surface into an "A" and "B" region, was biased in such a way that both under experimental and under control conditions the dice had a tendency to move into the A region. Throughout the reports making use of this table there is systematic alternation between use of the A surface and the B surface as the target area, and of course the successes are greater in the A area. The point, however, is that with proper alternation of A and B, the A is penal-

ized when nonpreferred just as it is favored when preferred.

Both series plainly show the favoring of the "A" side of the target already mentioned, an effect which could, of course, have been reduced by altering the apparatus, but H. F. did not consider it advisable to make the changes during the course of the experiment.

The fact that each side was used as target an equal number of times compensated for the bias in the apparatus.

Table I also shows the results of the two control series. It may be seen that although they gave a total deviation that was negative instead of positive (that is, the total number of dice falling into the target area was below the number expected on the chance theory), nevertheless the favoring of the "A" side over the "B" side continued about as markedly as before. This is what one would expect if the favoring of the "A" side in the experimental series was due chiefly to bias in the machine rather than to PK.

Yet it is in reference to this kind of reasonable comment by an experimenter that Girden finds it appropriate to berate the experimenter for inattention to equipment bias and for lack of control series. Girden's comment on this was that here was a patent flaw which should *at once* have been corrected. It seems to me that this is a question for an experimenter's judgment, regarding which outside observers or readers are hardly likely to have any great wisdom to offer. When Forwald, in a later experiment, continues to use the same table, he is reprimanded by Girden for this and reprimanded for not *re-stating* the fact that the table had a bias. Since, however, the same built-in controls are involved as in the earlier experiment, it would appear to me that this is a stylistic matter; the reader might well want to compare the two studies, noting that the same table was used. Later, when Forwald came to the United States, he built a new table which was free of bias, and

then both alone and with observers present he proceeded to get the same sort of placement PK effect that he had gotten in Sweden.

The Forwald data also yielded interesting material, replicated at least twice, indicating a U curve in the course of the hits in five consecutive tasks; that is, initial and terminal successes are commoner than those in the middle. Rereading these Forwald studies and rereading the Girden analysis, I cannot find what is seriously wrong with the experiments. What is featured is the fact that initially Forwald worked alone. What bearing this has on the situation in PK research today is not clear to me.

Incidentally, data from a control series following immediately upon experimental series are at times somewhat like the experimental series rather than like what is expected from a probability model. Is there not a possibility that there is a psychological perseveration from the task into the control, particularly if the longer one continues to gather the data, the less the control resembles experimental series? Of course, this has to be independently tested but it is a reasonable hypothesis as offered by Forwald.

Regarding this Forwald series as a whole, the use of hostile phraseology seems to call for some attention. Regarding the Pratt-Forwald (1958) experiment in particular, to which special importance has been attached (in which Pratt and Forwald worked through a long series of preliminary studies to see what the best psychological atmosphere was, then made predictions of a highly specific nature as to what the subject would do and then got the expected results), Girden comments: "In terms of planned experimentation, this study was free-wheeling, with no predetermined de-

sign and no recorders who were ignorant of the wished for area." In view of the fact that the correlations of observers in the Forwald series appear to have been of the order of .99, one cannot help wondering if the artillery used is somewhat heavier than required in scientific writing. It is certainly true that it would have been fine to get photographic records. I often wish that we had them ourselves for experiments in which we study the role of autistic factors in perception, where we find that such autistic factors as an expression of the experimenter's personality are nearly as dramatic as when they are an expression of the subject's personality. The absolute or ideal experiment has not, as already noted, been performed.

Thinking over this question whether you must always set up a fresh experiment if you are going to test a hypothesis that occurs to you after the event, I wonder how this would work in paleontology, or in the use of radioactive carbon to date historical events. It is not actually the prediction in terms of a fresh experiment that constitutes the ideal method, because the situation is not recoverable to this degree. Very often you go back over periods of time in which you think physical, biological, and psychological conditions are probably similar, and see whether you continue to get verification of a hypothesis which has occurred to you. If you can keep yourself uncontaminated, you may be able to find out something. When PK "decline curves," for example, were discovered, the aim was to go back and see whether they would recur in other material where they had not been suspected. This is not an ideal method, but it is an important step and hardly to be belittled. It is certainly true that the

common type of decline curve cannot be counted upon to reappear. There are, however, other types of decline curves, such as the rapid decline in effects which appear from properly randomized first calls or first blocks of calls as in Dale's work; these are of real psychological interest and call for much further work.

Again in the matter of decline curves, the fact that Forwald's outstanding result is with the first trial in fresh series, leads Girden to comment that "physical conditions were likely to be unsteadiest" during such first calls. Whether this is true, I am not enough of a physicist to say, but I am sure that there is a *psychological* difference between the first and subsequent calls, and I wouldn't want to pursue this as far as Forwald did (incidentally Forwald is an engineer). Girden also thinks that the dice throwing machine used by McConnell in the Biophysics Department at the University of Pittsburgh may, as the result of night usage, have been subject to a warm-up effect the following day. What would the warm-up effect do as far as decline curves in responses to randomized target orders are concerned?

The Girden report may have been prepared for publication sometime ago, for it makes no comment on several interesting recent reports on PK, notably one by Fahler (1959). Here the equipment last used at Duke by Forwald was used, alternating with each of the nine subjects from an A side to a B side of a dividing line in the middle of the table, so that subjects could wish for movement of the dice toward the one side or the other. As in Forwald's work, imperfections in the table or the rest of the equipment favoring one or the other side appear to be controlled by the experimental alternation between the two.

In the present experiment the same six cubes are used throughout the experiment, and whatever imperfections they contain must affect scores both in the five A-side trials and the five B-side trials which alternate with them. In Fahler's tests, preliminary work had shown which of the nine subjects were in general known around the laboratory as likely to obtain high scores in both PK and earlier clairvoyance tests, and which subjects could be expected to perform below chance level. The predictions were correct in eight out of the nine

cases, and when the scores achieved by all of the subjects are compared by *t* test, a significant difference between the positively predicted and the negatively predicted appears. Perhaps Girden would like to add a comment on this and several other PK papers which have been written within the year. It is true that no photographic record of positions of the dice are offered, but there is considerable interest, as there is in other recent papers, in the comparison of independent observers and the determination of a high order of agreement.

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A POSTSCRIPT TO "A REVIEW OF PSYCHOKINESIS (PK)"

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The opportunity has been afforded the writer to consider a *report* prepared by Murphy (1962), on "A Review of Psychokinesis (PK)" (Girden, 1962). Since no complete review of PK had previously appeared—in American Psychological Association journals, or elsewhere for that matter—the amount of detailed presentation required for a professional audience unfamiliar with the subject matter is not simply resolved. Space in scientific journals is always at a premium and its allocation is an editorial responsibility. In a specific editorial directive to condense the submitted manuscript, the printed *review* is a shortened version of the original manuscript and must be considered in this light.

How much detail is required to afford insight to the reader? Murphy (1962) comments that "I have said more than Girden says about the McConnell et al. studies and the Dale studies. Indeed, in many cases his accounts of experimental methods are so brief that it is hard to comprehend what *E* was trying to do, what his methods were, and what results he obtained." Yet with respect to the McConnell, Snowden, and Powell (1955) and the Dale (1946; Dale & Woodruff, 1947) studies, excluding the tabular description of experimental conditions and results, more than twice as many typed manuscript lines were used in the review by the

present writer than in the manuscript for the report. Further reflection at this time suggests that the presentation of McConnell et al. (1955), carried out in 1948-50, might have been further condensed since "the authors consider the *Journal of Experimental Psychology* article a preliminary statement to be followed by a more extensive report."²

Let us also consider the treatment of Binski's (1950) data, "on two exploratory series," which Murphy examines in detail. The text and Table 3 of the review demonstrate that all essential conditions were reported, including the number of *Ss* on the roulette test ($N=123$) and on the coin (100 per throw) test ($N=117$). Murphy emphasizes that one *S*, in coin tossing, scored "a very significant number of hits [548 out of 1,000] and he did as well or better in each of the next four throws." The deletion from my final version of the manuscript of the statement that this *S* was also superior on roulette guessing does not constitute a misrepresentation, since the review noted that some new unpublished tests with this *S* "were 'encouraging' but not statistically significant" (Girden, 1962). As far as is known, no further reports about this *S* have appeared. But the data at Las Vegas are more convincing in this connection. One player "held" the dice for about 1 hour and 20 minutes.³

¹ R. A. McConnell, personal communication, 1959.

² On a first throw, the total of the faces of a pair of dice, from 4 to 10 inclusive, constitutes a point to be made before 7 subsequently appears. If a 7 appears before the

³ Grateful acknowledgement is made for the Thomas Welton Stanford Fellowship (Stanford University) and the John Simon Guggenheim Fellowship which made possible this effort.

Consultation, as I have found, with gamblers where betting is legal or with any other devotees of this aspect of our culture will uncover even more startling data. Murphy apparently selects what appears to be a run of luck on two tests with *one S* out of 117 *Ss* as significant for PK.

One occupational hazard in preparing a review article is the failure to obtain complete coverage. The present effort has been convincing that complete coverage cannot be obtained from the published record. Indeed, I am much indebted to many people here and abroad who in personal conversations called attention to studies to which I had not seen reference as well as results not available in the published reports. It is not clear, however, why Fahler's (1959) paper was overlooked since the journal in which it appeared was one that had been thoroughly searched. I am therefore pleased that this unfortunate omission has been detected. But an examination of the results does not appear to change the basic issues.

This is not the appropriate place to deal with the problem of the psychology of scientific controversy, an interesting topic on which little research has been done (Boring, 1929, 1952, 1961). Psychical research, of

point, the dice are lost to the next player. If the point is made, the next "first" throw is the new point to be made, again before a 7 subsequently appears. (On a first throw, 7 as well as 11 is a point. Two, 3, or 12 appearing as a first throw results in the loss of the bet, but the dice are retained. The next throw constitutes a "first" throw.) As a result of caution, the player left after winning only a few thousand dollars.

which PK is one phase, is decidedly controversial. Here are expressions of opinion by Murphy (1948) regarding psychic research made in other contexts:

insofar as psychical research can ultimately qualify as an experimental science, it will be forced, as are all experimental sciences, to develop truly *repeatable* experiments; experiments which . . . can be independently repeated by any competent investigator. . . . It is hard to see even the beginnings of a science of parapsychology until a repeatable experiment . . . is obtained (pp. 18 f.).

And more recently he commented that:

The fundamental rule in laboratory science is that you truly have captured a phenomenon and begin to understand it only when you can so fully specify the conditions which engender it, that you can yourself make it happen again and again, and other qualified workers can do the same. We do not, for the most part, even attempt this in parapsychology (Murphy, 1958, p. 233).

These statements by Murphy appear to agree fully with the tenor of my review.

With respect to PK as well as extrasensory perception, the present writer's opinion coincides with that expressed by Murphy (1957), when he said:

I note a very marked petering-out of successful experimentation generally . . . not only is there a loss of spontaneous cases and mediumistic phenomena. The quantitative data themselves are sparse; slight effects have to be played up; large areas are barren; repetitions are rare . . . (p. 175).

Concerning the existence of PK, this writer has no strong opinion pro or con but, on the basis of the available evidence, the soundest judgment is a Scottish verdict: *not proven*.

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ERRATA¹

In the article entitled "An Exact Multinomial One-Sample Test of Significance" by A. Chapanis (*Psychol. Bull.*, 1962, **59**, 306-310) one of the terms in Equation 3, page 306, is incorrect. The equation should read as follows:

$$P = \frac{k!}{(2!)^{i_2}(3!)^{i_3} \cdots (i!)^{i_i} \cdots (k!)^{i_k}} \frac{N!}{n_1!n_2!n_3! \cdots n_j! \cdots n_k!} \left(\frac{1}{k}\right)^N$$

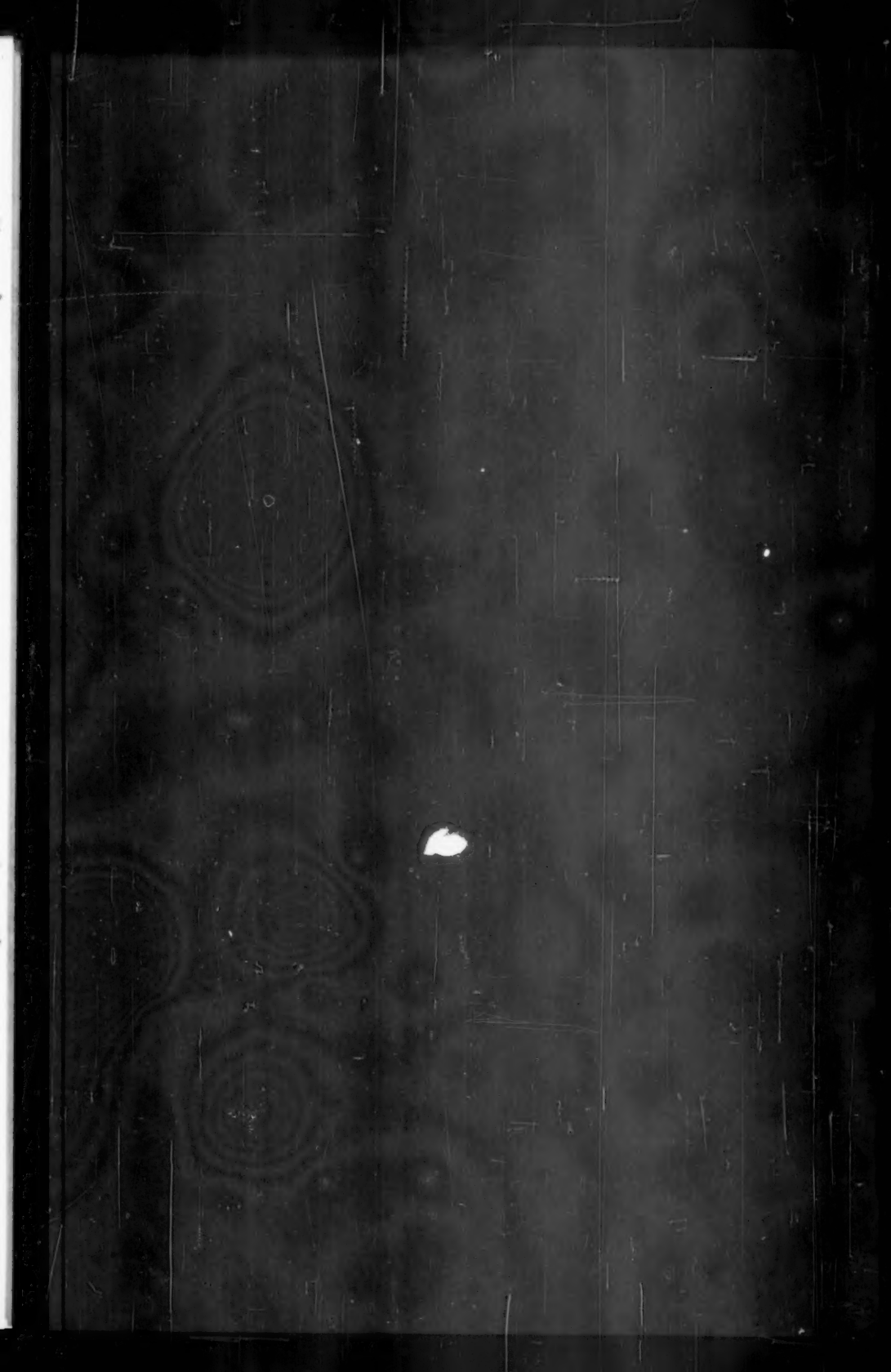
On page 307 the first line of computations illustrating the application of Equation 3 to a particular set of data should read as follows:

$$P = \frac{3!}{(2!)^0(3!)^0} \frac{12!}{8!3!1!} \left(\frac{1}{3}\right)^{12}$$

On page 308 delete the first two lines of computations illustrating the application of Equation 3 to another set of data. The computations on the third line of that illustration, and all other computations in the article, are correct.

In Equation 4, page 310, the quantity n_k should be read as n_j .

¹ The author is indebted to Joseph L. Fleiss, Biometrics Research, New York State Department of Mental Hygiene, for calling these errors to his attention.



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